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PERCEPTION, PHYSICS, AND REALITY;

AN ENQUIRY INTO THE INFORMATION THAT PHYSICAL SCIENCE CAN SUPPLY ABOUT THE REAL

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PERCEPTION, PHYSICS, AND REALITY;

AN ENQUIRY INTO THE INFORMATION THAT PHYSICAL SCIENCE CAN SUPPLY ABOUT THE REAL

by

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INTRODUCTION

THE present essay has as its object an attempt to discover how much natural science can actually tell us about the nature of reality, and what kind of assumptions it has to make before we can be sure that it tells us anything. By natural science, for the present purpose, I mean physics.

When a certain way of looking at the universe meets with the extraordinary success with which that of physics has met it becomes the duty of the philosopher to investigate it with care; for it is likely to offer a very much better Cosmology than his own unaided efforts can do. And, if philosophy is to take into account empirical facts—and it is extremely difficult to see what it will be able to tell us about the existent unless it does—it can hardly neglect the most fruitful and thorough investigation of certain large branches of empirical facts that has yet been made.

But natural science starts with certain assumptions, and, as it goes on, it developes certain general conclusions about the real world. For instance, it starts with a position not far removed from naïf realism, and, in its progress, it draws a distinction between the reality of primary and secondary qualities, and developes a causal theory of perception. Now the distinction between primary and secondary qualities as to their reality is a metaphysical question, and science seems historically to have taken over its answer to it from Descartes. This has led quite reasonably to an attack on science from later philosophers who have not agreed that it was possible to stop at the point at which Descartes and natural science stopped in this matter. If we look into the arguments of philosophers on this important question we shall, however, find that they have little right to cast stones, as their reasonings are confused and full of implicit assumptions which, when rendered explicit, tend to cease to be probable. It therefore becomes very important to reopen this discussion, and to consider what really is the truth about primaries and secondaries, and how far the particular traditional belief which science has inherited on this question is relevant to its real progress and is consistent with its other beliefs. I have therefore begun by discussing all the common arguments against naïf realism which do not depend on the belief that our perceptions are caused by real things identical with or rather like their objects. I come to the conclusion that none of these arguments which are so confidently repeated by philosophers really give conclusive reasons for dropping even the crudest kind of realism. I of course assume the distinction between a perception and its object which Mr Moore, in his *Refutation of Idealism*, showed to have been so largely ignored. Whilst the recognition of this distinction is all-important, and whilst it is perfectly true that many of the most plausible arguments used by philosophers against naïf realism depend on ignoring it, it is not true to say that *all* arguments for Idealism rest on this confusion. Its recognition is perfectly compatible with the belief that the objects of none of our perceptions continue to exist when we cease to be aware of them. And the arguments that I discuss in my first chapter are attempts to prove this proposition, at any rate for some objects. Whilst I do not think, as will be seen, that they do absolutely *prove* anything of the kind, it is not true that they rest on the confusion that Mr Moore pointed out.

In the second chapter I discuss Causality, with particular reference to the objections that have been brought against causal laws on various grounds by philosophers like Lotze and Mr Bradley. It is clear that, unless causal laws have some kind of truth, science in general cannot tell us about the nature of the real; and, in particular, we cannot investigate arguments against realism that rest on the belief that our perceptions are the products of causal laws that include our minds, our bodily organs, and events in a real world. I have been much assisted here by the views on Causality in Mr Russell's *Principles of Mathematics*, where they were relevant. At the same time I think that they suffer by having been stated with particular reference to the science of mechanics. Mechanics is so abstract and has been so successful that by keeping it too much in mind we are liable to forget the difficulties that beset causation in other regions where we certainly suppose that it holds. I have been led in the end to state causal laws in terms of probability.

I must say a word at this point as to the use of probability in this essay. I have constantly put my conclusions in terms of probability and not of certainty. This will perhaps seem peculiar in a work which claims to be philosophical. It seems to me that one of the most unfortunate of Kant's *obiter dicta* is that philosophy only deals with certainty, and not with probability. So far is this from being the case that to many philosophical questions about the nature of reality no answer except one in terms of probability can be offered; whilst to some there seems no prospect of an answer even in these terms. Few things are more pathetic than the assumption which practically every philosopher[1] makes that his answer to such questions is the unique possible answer; and few things are funnier than the sight of a philosopher with a theory about the real and the nature of perception founded on numberless implicit assumptions which, when made explicit, carry no conviction whatever, telling the scientist *de haut en bas* that his atoms and ether are mere economical hypotheses. Into the meaning and nature of probability I have entered as little as I could. This is not because I underrate its importance, but because I do not feel competent to attempt a task which has happily fallen into better hands than mine. But, as there is little question about the actual laws of probability, this omission is less important than it would otherwise have been.

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[1] Cournot is an honourable exception.

In the third chapter I discuss a form of the doctrine known as phenomenalism. This theory is commoner among physicists than among philosophers, which is rather ominous for a philosophic theory. The reason for the wide belief that it enjoys in this quarter is that the physicist who is interested in the philosophy of his science naturally turns to the works of Mach and his school. I should be the last to deny the many excellencies of those works; but they do not form an adequate philosophic equipment for the decision of the questions which they so confidently solve in the phenomenalistic sense. I have therefore devoted a very short chapter to what seem to me to be the essential points in the discussion of phenomenalism. This is to be taken mainly as prolegomena to the fourth chapter on the causal theory of perception. Here I have discussed as fully as I could this theory, which nearly everyone assumes, and which has been so little discussed in its purity. It is curious to note how often philosophers seem to have held that, when they have offered an explanation of the causes of our perceptions of certain objects or of the origins of certain beliefs, they have proved that those objects cannot be real and that the beliefs must be false. This has been particularly the case in the matter of perceptual and conceptual space. An honourable exception to this general failing is Lotze, who very justly points out that presumably all our beliefs and perceptions have causes, whether they be valid beliefs and perceptions of real objects or not. I have tried to clear up the relation between the causes of our perceptions and the reality of their objects; and have been forced to conclude that, in all probability, the objects of our perceptions do not exist when they are not perceived, although there is no absolutely conclusive proof of this. The question then arises as to whether this ought to reduce us to complete agnosticism about the nature of the real causes of our perceptions. I have tried to show that the scientific account of the causation of our perceptions can be so stated that its success will enable us to gain probable judgments about the nature of those real causes, in spite of the fact that it seems to be stated in terms of what have been seen to be, in all probability, mere appearances.

I have concluded with a discussion of Newtonian mechanics. This may seem to be out of order in a work which is primarily philosophical, especially at a time when there is some reason to think that the classical mechanics is only a particular case of electromagnetic laws. But the philosophical problems raised by the laws of the motion of matter are essentially the same as those raised by the laws of the motion of electricity. These problems are that of absolute or relative motion, and that of the reality of force. And it is clearly better for philosophic purposes to discuss these questions with reference to the ordinary laws of motion which everybody knows and which are clearly applicable to a very large class of motions, if not to all, within the limits of experimental error. I have been most helped here by the relevant chapters in the *Principles of Mathematics.* It will be seen, however, that I do not think that Mr Russell proves his point about absolute space.

The greater part of the present work was submitted as a dissertation at the examination for Fellowships at Trinity College, Cambridge, in 1911. I am far from thinking it conclusive, and I still hanker after a more realistic view than it reaches. But, if it succeeds in showing old arguments and old difficulties in a clearer light, and in bringing forward latent assumptions, it will not have failed in its object, and may serve as a stepping-stone to further investigations.

I have to acknowledge but little direct indebtedness to books, except in the last chapter. But I owe much to the lectures and conversation of Mr Bertrand Russell, and should have found his little book on Philosophy useful if it had appeared when I was writing. I also desire to express my obligations to the lectures of Mr W. E. Johnson of King's, whose rooted antipathy to publishing anything till he is sure of everything is a great misfortune to philosophy. I must also thank Prof. Stout for kindly reading the proofs, and for making many suggestions and criticisms. Sometimes I have modified my argument, or made additions, to meet points raised by him; and when I have not done so, it does not mean that I have failed to see the force of his objections.

C. D. B.

CAMBRIDGE,
September, 1913.

UNIV. OF CALIFORNIA

CHAPTER I

ON THE ARGUMENTS AGAINST NAÏF REALISM INDEPENDENT OF THE CAUSAL THEORY OF PERCEPTION

In the present chapter we are going to begin from the position of naïf realism. It is true that our everyday view of the world is not quite naïvely realistic, but that is what it would like to be. Common-sense is naïvely realistic wherever it does not think that there is some positive reason why it should cease to be so. And this is so in the vast majority of its perceptions. When we see a tree we think that it is really green and really waving about in precisely the same way as it appears to be. We do not think of our object of perception being 'like' the real tree, we think that what we perceive is the tree, and that it is just the same at a given moment whether it be perceived or not, except that what we perceive may be only a part of the real tree.

But, as everyone knows, we do not stay in this happy condition of innocence for long. We have perceptions which are believed to be illusory by which it is meant that their objects only exist when they are perceived. Common-sense does not believe in the reality of what is perceived in dreams or delirium. It applies certain arguments which it thinks are fatal to these. But as soon as it has rejected these objects it needs some account of the way in which the perceptions of them are produced. This leads it to a causal theory of perception, and when this is worked out it leads further still away from naïf realism. We shall discuss the causal theory and the arguments based upon it in a later chapter. Here we want to consider those that are independent of it.

Although in a sense naïf realism stands at the opposite pole of thought to phenomenalism, there is another sense in which it resembles it much more closely than any intermediate philosophic position about the reality of objects of perception. The point of difference of course is that the naïf realist maintains that in perception we are directly aware of what would equally exist and be unaltered in any of its qualities at the moment at which we perceive it even if we did not perceive it, whilst the pure phenomenalist holds that what can be perceived must be perceived, and that nothing exists except mental states and the objects of those of them which are perceptions so long as they remain objects of them. The point of resemblance is that neither the phenomenalist nor the naïf realist can admit appearance or illusion. For both everything is precisely as it appears; the only difference is that the phenomenalist holds that all that exists must appear and that it ceases to exist when it ceases to be perceived.

Before we consider the particular arguments which are supposed to make it inevitable to desert naïf realism we must consider what precisely it is that those arguments are supposed to prove. They want to prove that some at least of the objects of which we are immediately aware cease to exist when we cease to be aware of them. Naïf realism is the denial of this proposition. There are different degrees to which this belief is dropped and we want to see how far, if at all, arguments show us that it ought to be dropped. Thus common-sense would say that a mirror-image of a pin only exists when someone sees it, if, by the mirror-image of a pin you mean something that looks exactly like the sort of pins that we can feel and with which we can scratch our fingers. On the other hand it would say that the pin whose reflexion is the mirror-image exists whether we perceive it or not precisely as we do perceive it when we do so. Grammar? But natural science would say that what we perceive in this latter case even is not what exists when we do not perceive it, nor even particularly like it. There is no reason, it would hold to believe that it has a colour or a temperature, but most likely it consists of little hard colourless things of uncertain shape vibrating rapidly and never getting far away from each other. Idealism again would say that what is real is not in the least like what we see, but that it is a thought in God's mind, or a community of spirits of low intelligence.

Now there is one very important point in all this, which is often overlooked. This is that whatever else may or may not exist it is quite certain that what we perceive exists and has the qualities that it is perceived to have. The worst that can be said of it is that it is not also *real*, i.e. that it does not exist when it is not the object of someone's perception, not that it does not exist at all. When I see a pin that of which I am immediately aware is neither colourless atoms nor a community of spirits; and this is a matter of simple inspection. But it is also quite certain that the objects of existent perceptions exist at least so long as the perception of them does so. Hence on any tenable view of the world there exist things in it that are coloured and hot and extended. The only further points of interest about these qualities are (*a*) whether they can ever exist except when someone is immediately aware of them, since it is quite certain that they do when people are aware of them and that many folk are aware of them from time to time; and (*b*) whether there is anything in the nature and quality of these objects of immediate awarenesses which justifies a belief in the existence of other things of which no one is immediately aware, which differ more or less from those objects, and yet have a peculiar relation to them which I shall at present denote by the purposely vague phrase 'correspondence.'

We saw that common-sense accepts naïf realism wherever it is not forced by arguments to abandon it, and we can now see some of the general conditions to which all special arguments must conform. In the first place we must be able to assert that, if *p* be any quality the attachment of which to any subject *A* is a mark that the latter cannot exist unperceived, it must not also be fatal to *A*'s existence when perceived *p* must only be fatal to the unperceived existence, i.e. to the reality of *A* and not to *A*-as-an-object-of-perception; since it is solely by examining the perceived *A* which exists that we find that it has the quality *p* in question. It is not valid to say that, in reply to this, we might argue: Perceived *A* has not the quality *p*, but if *A* were unperceived it would have *p*, and therefore *A* cannot exist unperceived. For, if it has a new property when unperceived it is not *A* the possibility of whose unperceived existence is denied but something else *A′* which differs from *A* in another respect beside that of being unperceived.

I do not know whether this line of argument will be considered shocking or truistic. I think it is merely the latter; but it is certainly worth insisting upon, since in these matters truisms are disregarded as the necessity for Mr Moore's 'Refutation of Idealism' showed. The great service of that article is to insist on the truism that when you perceive you perceive something, and that what you do perceive cannot be the same as the perception of it. And Mr Moore was undoubtedly right in saying that the recognition of that truism is a complete refutation of most of the current arguments for idealism.

Let us then apply this general reasoning without undue nervousness. I will take an example. We all perceive extended things in spatial relations, and these perceptions give rise to a series of judgments about extension. The hundred and one arguments against the reality of space independent of the causal theory of perception consist of attempts to show that when you reflect on these judgments they are found to be in contradiction. Now these arguments may be true or false:—they are all so far as I know false. But let us suppose that one at least of them is true. Then we must admit that nothing extended exists unperceived. But, since it is notorious that we do perceive extended objects, and since it was only by reflexion on the qualities and relations of such objects that we arrived at the judgments that are now proved to contradict each other—we must also admit that, if our argument be correct, something must exist—at any rate as the object of an existent perception—which has characteristics that lead to contradictory propositions, even if it be granted that such things could not exist except as objects of perception. In fact we must not say that a perceptible quality that leads to mutually incompatible judgments about itself or that of which it is a quality cannot exist, but that it cannot be real, i.e. cannot exist otherwise than as an object of perception.

If such cases really arose we should have to come to some compromise between three very strongly held beliefs: (1) that we cannot possibly be mistaken in our belief that the objects of some of our perceptions have that peculiar characteristic which we call extension, (2) that we cannot possibly be mistaken in the propositions about extension which we come to believe in reflecting on such objects of perception, and (3) that contradictory propositions cannot be true even of something that only exists when perceived. It seems to me that (1) and (3) must be accepted, but that it is only under certain special circumstances which we shall mention later that we are obliged to accept (2). Hence if it were true (which it is not) that the standard arguments proved that propositions which we believe about extension are mutually incompatible, the right course would be neither to reject the reality of extended objects, nor to hold that objects which give rise to incompatible propositions about themselves can exist when perceived though not otherwise, but to conclude that some of the beliefs which we reached by reflexion on such objects were mistaken.

It is obvious that the main general difficulty of the arguments that we are going to discuss in this chapter will be to find propositions on which every one will agree that if they be true of an object of perception then it cannot exist except when perceived, propositions which at the same time are admittedly true of what we do perceive. The fact that the simple point which I have laboured perhaps too much above has seldom been grasped or explicitly stated is the cause, I think, of the unsatisfactoriness of most of the arguments against secondary qualities. After reading the arguments one is left with an uneasy feeling of puzzlement. What precisely is it that they do prove? Do they prove or only render probable? And must they not assume many more axioms than their users make explicit to prove as much as they seem to be supposed to do? Our best plan then will be to go over the main arguments *seriatim* and to try and make explicit those propositions that must be assumed if they are to be logically valid grounds for believing that their conclusions are certainly or probably true.

Before we discuss the arguments in detail it will be well to define the meanings that we intend to attach to certain terms which we shall constantly use. The word object is ambiguous. When I perceive a rainbow or a chair in waking life and when I perceive anything in a dream all that I perceive can equally be called objects. I intend to use the word in the general sense of anything that may be perceived, regardless of the question whether it can exist if it be not perceived. There is a narrower sense of the word which would confine it to objects that are supposed to exist whether they are perceived or not. Thus the dream chairs and tables would not be called objects at all, nor would the colours of the rainbow; but the chair perceived in waking life and believed to exist whether perceived or not would be called an object. In the case of the rainbow it would be quite in accordance with usage to say that the object of the perception of the rainbow was little drops of water with light shining on them. This use is very common where the object of perception in the wider sense is believed to be very much like the object in this narrower one and to have qualities which vary continuously as we move about and which, in some limiting case, coincide with those of what exists whether we perceive it or not. Thus suppose there is a real circle. This will be an object in the narrower sense, since it will continue to exist and be circular whether we perceive it or not according to the usual opinion of common-sense. As we move sideways however what we really perceive is a series of ellipses of continuously varying eccentricity. But if a man standing sideways to the figure were asked: What object do you perceive? he would almost certainly answer: A circle; because he believes that what exists independently of his perception has the quality of circularity. Hence it is not uncommon to call the object of perception in such cases not the object that is actually perceived but the object that is inferred from what is perceived to exist whether it be perceived or not.

This confusion has been responsible for much error and it is necessary to be clear about it before we can go on. We shall continue to use 'object' in the wider sense to cover anything that actually is sometimes immediately perceived, whether it exists when we cease to perceive it or not. We shall divide objects into appearances, which only exist when they are perceived, and realities, which exist unchanged whether perceived or not. All appearances are objects, but it does not follow that all realities are objects. For we might have grounds for believing in the existence of realities which we never could directly perceive. Such realities would be a light-wave or an electron. Thus we must accept the possibility of real non-objects, though there cannot be apparent non-objects. When I do not want to beg the question of whether we are dealing with an object or a non-object I shall call it a term, but we shall not often want the word.

We shall now allow common-sense to assume that naïf realism is true in all cases where there is no reason to believe that it is false, and consider the reasons other than the causal theory of perception that have led people to suppose that in many cases it must be rejected. This will be the easier as the type of argument is exemplified in the rejection of secondary qualities by Locke and Descartes, and of primaries by Berkeley and Bradley. The reasons that have been offered for the rejection of the reality of certain sense-qualities have been numerous. The following list makes no claim to be exhaustive, but I do not think that it misses any argument of importance. (1) Spatio-temporal incompatibility (e.g. the hands in water experiment). (2) Conflicting testimony of two different senses in the same person. (3) Conflicting testimony between the same sense in two different people. (4) Perceptions in dreams and morbid bodily states. (5) Qualities like temperature merging into feelings of pleasure-pain. (6) Relativity of what is perceived, to the position and past history and present structure of a bodily organ and to the use of instruments of precision like microscopes. We can really group (1), (2) and (3) together as far as the main principle on which they rest is concerned, though their differences will demand separate discussion. The point of agreement is that in all, the senses testify to the existence of combinations of characteristics in one spatiotemporal point which are known to be synthetically incompatible. (4) and (6) are in the main independent of each other but not entirely so. Morbid bodily states may be produced by drugs and are then supposed to cause illusions; but what is perceived by means of instruments of precision is also caused by extraneous means acting on the body, yet this is supposed to add to our knowledge of the real. Again (4) and (6), and more especially (6), are chiefly concerned with causation of perceptions. As such they will be largely reserved for a later chapter. We can then put our final list of arguments in the following scheme.

I. Synthetic Incompatibility
  (*a*) Testified by one sense in one person.
  (*b*) Testified by different senses.
  (*c*) Testified by one sense in different people.

Mainly causal
II. Relativity to organs of perception and the use of instruments of precision or drugs.
III. Occurrence in dreams or morbid bodily states.
IV. Merging into feelings.

I shall begin by trying to show that I (*a*) which has generally been accepted and is almost the only argument used by Locke rests on a confusion and is incapable by itself of proving or rendering probable what it has been supposed to do. For this purpose I shall take Locke's example of the two hands in water which is supposed to disprove the reality of temperature.

As everyone knows, in this experiment the two hands have been treated differently, the one having been plunged into a vessel of cold water and the other into one of hot water. They are then put into a third vessel of water at temperature intermediate between the other two, and the water in this appears to the cold hand to be hot and to the hot hand to be cold. Hence it is argued that the water cannot really be either hot or cold, for if so it must be *both* hot and cold at the same time, and this is held to be impossible. As it stands then this is an argument from synthetic incompatibility and is an example of I (*a*). But in as far as what is perceived depends on the past history of the organ it is an example of II.

The moment we consider it carefully we shall see that its weakness is deplorable. (i) In the first place it is to be noted that it cannot make it even probable that two bodies at least—viz. my two hands—are not really hot or cold. It is evidently not true that I cannot have contemporary perceptions of different temperatures. It is notorious that I do, and that is indeed one of the data on which the argument is based. The argument rests on the proposition that no body can have different temperatures at the same time and place. But how could this proposition have been discovered or proved if as a matter of fact no bodies have any temperature at all? It would have to be held that we are immediately certain of the following proposition: If (*per impossibile*) bodies could have real temperatures they could not have two different ones at the same place and time. The fact is however that the argument assumes what is no doubt true that I can never *feel* the different temperatures at the same time and place. But I localise the temperatures that I feel on my skin, and, since I never do feel the same part of my body to be at different temperatures at the same time, no experiment can give me any reason to think that my own body, at any rate, cannot have temperature[1]. Thus the argument gives no reason for thinking that my two hands in Locke's experiment are not really hot and cold respectively as I perceive them to be, and that *caeteris paribus* they might not continue to be so when I cease to perceive their temperatures. (ii) It is clear then that the argument cannot apply to the temperatures that I directly perceive, viz. those of my hands, but only at best to inferences from these to the temperature of the surrounding medium. Now, if I make the particular assumption that the temperature of the water is that of the hand with which it is in contact, it might be thought that the argument will be valid as a proof that the water has no real temperature. But once more the assumptions made are inadequate, and that in several respects. (*a*) If two propositions together lead to a false conclusion you have no right to reject one rather than the other on these grounds alone. Hence if the experiment + the assumption that water has temperature + the assumption that the temperature is of the same degree as that in the hand did lead to the impossible conclusion that the water has different temperatures at the same time and place, this cannot be said to disprove the assumption that it has temperature rather than the assumption as to the magnitude of the temperature if it has it. One assumption would have to go, but the experiment throws no light on which it should be. (*b*) But, as a matter of fact, neither assumption need go, and that for a very general if empirical reason which the supporters of this kind of argument seem to have overlooked. That reason is that the conditions which the experiment requires to be satisfied never are and never can be fulfilled. Either the fingers touch or they do not. If they touch, the water is not felt but the fingers. If they do not, then they are not at exactly the same point in the water. Nobody in the world ever has had his two hands at the same time in the same place, and we may say with some confidence that nobody ever will perform that feat. But, failing it, the whole argument falls to the ground. The experiment + the assumptions can now only prove that at the moment at which it was tried the water had different temperatures at two points very close together. And there is no objection empirical or *à priori* to such a state of affairs. (iii) Supposing that all these difficulties could be set aside (which they cannot), how could the right conclusion be that the bodies have *no* temperature? We have seen that some bodies, viz. our own, must have temperatures at any rate while we perceive them. For we do perceive them and we do perceive them on our skins. And there is nothing to lead us to suppose that the temperatures of our bodies might not exist even when not perceived. Hence it seems much more reasonable to conclude from the experiment that, though other bodies have temperatures, we cannot tell precisely what their degree is from that of the felt temperature of our hands, than that other bodies have no temperature at all and our own none except when we perceive it.

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[1] The only paradox here is that it is the hand whose past treatment might have been expected to have made it the hotter which is actually the colder and *vice versâ*. But nothing is *proved* by this paradox.

Most of the arguments that Berkeley employs are vitiated by a certain fallacy which would prevent him from taking the alternative that we have suggested as the more reasonable one in view of the experimental results and the other facts that are known. This fallacious argument is the following one. He says, what is perfectly true, that those sensible qualities that have intensive magnitude, for instance, cannot exist in general but can only do so with some definite degree of that magnitude. But his ridiculous doctrine about abstract ideas leads him to conclude from this that it is impossible to hold that a certain quality exists with some intensive magnitude although one does not know what the particular degree may be. There is of course nothing impossible or unreasonable in combining the belief that such qualities can only exist in definite degrees with the belief in the existence of such a quality whose definite degree we are unable to determine although we know that it must be determinate.

We may then sum up the objections to I(*a*) in as far as it applies to qualities which are perceived as localised on our bodies and extended. (i) You cannot directly *perceive* synthetic incompatibilities. Hence the perceived qualities with which the argument starts cannot be synthetically incompatible. There is therefore no reason why they should not be real qualities of your body at any rate. Hence synthetic incompatibility with regard to the deliveries of one sense whose appropriate sense-quality is localised on the body cannot possibly prove that what is directly perceived there might not continue to exist even if it ceased to be perceived. For there can be no reason to suppose that what was synthetically compatible while perceived, will suddenly become synthetically incompatible when you cease to perceive it. (ii) The argument can therefore only apply to what is inferred to exist in other bodies from what you perceive to exist in your own. But then the argument alone will not tell you whether you ought to reject the belief that the quality exists unperceived in these other bodies, or the belief that its magnitude is related in such and such a way to that of the perceived quality in your own body. (iii) The Berkleian objection, to the possibility of holding that a quality can be known to exist with a definite intensive magnitude whilst we cannot be sure as to the precise degree of that magnitude, is invalid. (iv) Finally, owing to the fact that our organs of sense are extended and incompenetrable, the common assumption that, in the case of those that act by contact, the quality is the same at the point touched as at the touching point of the organ can never be strictly refuted by this line of argument.

I have assumed in the discussion of the above experiment that the quality of temperature is directly localised in our skins and then inferred to exist elsewhere. I believe this to be true; but, for a general criticism of this line of argument, we want to see what will happen if we remove this restriction. I do not think it will make any essential difference. It is necessary that the qualities to which the reasoning from synthetic incompatibility is to be applied shall be directly localised *somewhere*, for otherwise their spatiotemporal incompatibility is meaningless. And at that point, whether it be in our organ of perception or in a foreign body, it is impossible that there should be synthetically incompatible qualities, since the qualities there are directly perceived, and you cannot perceive incompatible qualities at the same time and place. Hence as before it will always be a question of arguing from perceived and therefore compatible qualities to unperceived ones that are supposed to be incompatible and, wherever the perceived qualities that are the data of the argument be localised, the same criticisms will apply as we have enumerated above.

I think it will be worth while to say a word or two about localisation before leaving this subject. It is obviously all-important in questions that depend on synthetic incompatibility, and it is less simple than it seems. What precisely do people mean by saying that different degrees of temperature (e.g.) cannot coexist at the same point? Of course the hands in water experiment with its usual carelessness asserts the obvious falsehood that the same body cannot be both hot and cold at the same time. In the previous discussion we made this precise by replacing it by 'different degrees of temperature cannot coexist at the same point.'

This statement seems obvious enough, but it is by no means free from difficulty. When we talk of 'the same point' in what space precisely do we suppose the point to be? The spaces revealed by our various senses cannot be immediately identified, and what we call Space is a synthesis and construction with these as data and elements. When we say that two different degrees of temperature cannot coexist at the same point, do we mean 'at the same point in the space of the sense (viz. touch) by which we perceive temperature' or 'at the same point in the supposed common Space'? And we have two further alternatives before us. Either the proposition is supposed to be founded by induction on experience, or else it is *à priori* in the sense that it is based on the mere contemplation of the nature of temperature. If the proof be inductive and it takes the form that we never have experienced two different degrees of temperature at the same time in the same point of tactual space, and therefore they cannot coexist there, it is singularly weak. It may be a good enough argument to render it probable that we never shall *experience* different temperatures together at the same point, but it cannot possibly prove that they do not coexist. For an alternative explanation is that we are only capable of perceiving one of them at a time; for instance it might be always the highest of several temperatures coexisting at the same point that we perceive. For if the principle is to be used against people who believe that unperceived temperatures may exist, it must not assume that they are wrong in order to prove itself.

But I think that a good many people would say that their belief is not founded on induction from experienced lack of coexistence, but is *à priori* in Meinong's sense, i.e. although it is in perception that we become acquainted with temperatures, yet, once acquainted with them, we can see that from their very nature it is impossible that different degrees should exist contemporaneously at the same point. This position is at any rate much stronger than the one which we have just discussed. We saw that that would not even prove the proposition for tactual space, *à fortiori* experienced lack of coexistence in tactual space will not prove absence of coexistence in an assumed common Space. But the *à priori* certainty, if it exists at all, may be very general; it may even take the form that[1] quantities with different intensive magnitudes of the same kind which are also extended cannot exist contemporaneously at any point of any space.

So far as I can see the statement in terms of points, like points themselves, is the child of abstraction and intellectual construction; it is convenient but may not correspond with anything that is real, and ought for the present purpose to be avoided. The more concrete form of statement is that no volume in any space can be wholly occupied at the same moment by each of two quantities possessed of different intensive magnitudes of the same kind. Of course, if this principle be really *à priori* there is nothing further to be said; but is it?

We must remember that our principle is supposed to apply not merely to intensive magnitudes but also to shades of colour which are not quantities that differ in intensive magnitude, but are capable of arrangement in a continuous order. It will be best for the moment to confine ourselves to colour because there is no question here of anything corresponding to physical conduction as in temperature. Suppose we have two closed surfaces, one red and one blue, and that they are not initially in contact. Now I do not pretend to know exactly what common-sense means by a surface, but two possibilities are open. Either there are or there are[1] not outermost points to physical surfaces. There is no means of deciding this question; but we can at least say that it is arbitrary to suppose that some surfaces have and others have not outermost points. Now let the two surfaces be brought into contact. I do not know exactly what common-sense means by this, but I take it to mean that a point of contact is either an outermost point of one surface or an outermost point of the other. If then neither has outermost points they cannot be in contact. But we suggested that it is reasonable to suppose that if either has outermost points both will have them. Hence if there are points of contact they must be outermost points of both. But the points of one surface are red and of the other blue; hence at the points of Space occupied by the points of contact red and blue will coexist[2].

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[1] I use the terms quantity, magnitude, and kind, in the same sense as Mr. Russell in his *Principles of Mathematics*.

If this argument be sound, common-sense must reject or modify its alleged *à priori* principle, 2 or deny that coloured bodies are ever really in contact, 3 or introduce a new physical law *ad hoc*, 4 or reject the reality of colours. With regard to the last three alternatives a few words must be said. There is no evidence that bodies ever are in contact with each other in physical Space unless we suppose that the denial of *actio in distans* is another *à priori* axiom. The latter seems to me to have no self-evidence, whilst the axiom about synthetic incompatibility has considerable plausibility. And, if we admit—what seems reasonable—that in the object of a present perception there may be distinctions that are not perceived, there would be a meaning in the statement that no bodies are in contact in perceptual space too. By the introduction of a physical law *ad hoc* I mean that we might suppose that it is a law of nature that as two coloured outermost points approach each other each modifies the other's colour so that they become more and more alike, and at last when they coincide their colours are identical. You may say that this is not a very probable law on the ground that it seems to treat outermost points in a different way from others, no matter how near them. But there is no need to assume this. When these coloured bodies approach each other all points in the one may influence the colours of all points in the other; but if the influence decreases very quickly with the distance everything will appear unchanged. With these two alternatives open to us it would be foolish to deny the reality of colours on the ground of the principle and the apparent fact that differently coloured bodies can come into contact.

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[1] I need scarcely mention that a finite volume and a definite shape are perfectly compatible with the absence of outermost points.

[2] Of course other meanings of contact are possible. If bodies have no outermost points we might say that *A* and *B* touch at *P* when every point on one side of *P* is red and belongs to *A* and every point on the other side of *P* is blue and belongs to *B*, whilst *P* has no colour. I merely want to illustrate that common-sense is not clear as to precisely what it means and that an alleged *à priori* certainty is of little value under such circumstances.

I suspect however that common-sense would be inclined to keep the possibility of contact and to introduce no physical law, but to modify the principle in the direction that it is no matter that different colours should coexist at the same point of physical Space so long as they are the colours of different bodies. But, until common-sense can give some account of what it means by the same and different bodies which shall render plausible the distinction that it is now trying to introduce, it is hardly worth while to discuss this modification.

If the above discussion has accomplished nothing else it has perhaps served to show that the alleged *à priori* principle is by no means so simple as it looks. I do not wish to deny that it may be *à priori*, but I wish to suggest that it can hardly be used with confidence as a metaphysical criterion of what may and what cannot be real. For an *à priori* judgment is one where, given the terms, we are supposed, by contemplating their nature and without further appeal to experience, to be able to make assertions about some of their relations. And we cannot place much trust in such judgments when a little discussion shows that our minds are in a most nebulous state as to the nature of the terms and the exact meaning of the assertions about them.

To conclude with some rather more positive considerations about the alleged *à priority* of the principle of synthetic incompatibility, I must confess that when I ask myself why I do not believe that two different temperatures or two different colours can contemporaneously fill the whole of any one volume, I am not contented (as I ought to be if the judgment were *à priori*) to refer merely to the nature of temperature or colour or intensive magnitude as such. I rather say to myself: 'If the different temperatures ever did exist they would at once be equalised by conduction,' and 'if I put one pigment on top of another they mix to a new colour.' But these are empirical laws which may be either laws of the physical world or of the limitations of our perceptive faculties. In either case they may have exceptions, and in the second case, even if there are no exceptions there is no proof that different colours and temperatures do not really coexist. Thus if there actually were experiences which, on the assumption that temperatures or colours are real, would force us to conclude that different temperatures or different colours can fill at one time a single volume, it is not clear that they would necessitate the rejection of the reality of colours or temperatures, since they might be interpreted as examples by reasoning from which we could prove that an alleged physical law has exceptions or that our perceptive faculties have limitations. And neither of these results would be so startling as to compel the rejection of the hypothesis on which they are based.

Since the argument from synthetic incompatibility thus assumes that the qualities to which it applies are extended and localised there is only a certain number of qualities to which it does apply. These are temperatures, pressures on the skin, colours, and flavours. It does not apply to sounds, because these are not generally localised in the way that we might at first suppose, and moreover are not synthetically incompatible. When we hear a bell tolling we might be thought to localise the sound in the bell, but this is not really comparable with localising temperatures on our skins or colours in definite places. It is impossible to feel temperature without feeling a definite hot (or cold) surface, or to perceive colour except by perceiving a definite coloured surface :—this is even so when we press the eye and see stars or rings. But it is perfectly possible to perceive a sound without localising it in that body that causes it. When we do think of extension and position in respect to sounds it is not of a volume that has the quality of sound but of one that is in a state of 'sounding,' i.e. of being the cause of the sound or of the perception of it. But we never think of the surfaces that we perceive as coloured, as being not really coloured, but only the causes of the colours. For, even if we adopt the ordinary natural scientific view about colours, it is quite clear (*a*) that we perceive colours as extended, and (*b*), granted that this perception is caused by surfaces and volumes as sound is believed to be, it cannot be by these surfaces and volumes that we do perceive, for they at least *are* coloured whilst science denies that the causes of our perceptions of colour are coloured. If sound as such be a quality it is perceptually and immediately localised in the space between our ears and the sounding object, and there is no synthetic incompatibility, for that space can be contemporaneously filled by all kinds of different sounds.

I think we may now claim to have shown that arguments of the type I (*a*), i.e. arguments from synthetic incompatibility alleged to be testified by one sense can never by themselves prove or render probable the unreality of the qualities against which they are directed. We can pass therefore to

I (*b*), i.e. to the denial that a quality can exist unperceived owing to the synthetic incompatibility of the testimonies of two different senses. There is only one set of sensible qualities about which more than one sense is held to be capable of giving direct information. Those are the geometrical qualities like shape and extension, which are perceived by both sight and touch. Now, as everyone is aware, sight and touch can be very discordant in their deliveries, and that in more than one way. For instance, two cases of what looks cubic to sight are sometimes found on being touched to give widely different tactual and muscular sensations. For example, what we should commonly call a 'real cube' will give tactual perceptions of eight corners, twelve edges, etc., whilst the picture of a cube, which may look exactly like the former, will merely be felt as a flat surface. If then it be held that in sight and touch we become aware of the same reality, then the reality of which we become aware cannot in general be the common object of sight and touch, since we have seen that there is often not a common object to the two perceptions, as distinctions can be found in the one that cannot be found in the other. We are sure that the same object cannot be both a plane surface and a three-dimensional figure, and therefore, even though sight and touch be indications of the same object, that object cannot be what is directly perceived in both of them and may not be what is directly perceived in either. In the same way, of course, you can get tactual perceptions of three-dimensional objects to which visual perceptions of two-dimensional ones correspond, as when you look straight on to one side of a cube and merely see a square surface, whilst you can still feel the corners and edges and other faces.

Now what precisely do these facts taken by themselves prove? It has always been recognised that the perception of extended shapes in definite positions is a much more complicated affair than the perception of the objects of other senses. By this I mean that, although there is of course a large perceptual element in it, there is also a great deal of judgment, association, analysis, and subsequent synthesis involved which are not present to anything like the same extent in what are called the 'perceptions' of the other senses. We want to separate off the perceptual element from that of judgment in order to determine precisely where the common element in perceptions of touch and of sight lies.

It is sometimes held that perception cannot be defined except by reference to its supposed causes. This, I think, would be a dangerous doctrine. Of course there is in it an element that is indefinable, viz. that of direct awareness of an object. But, then, many people think that there is direct awareness of something in all kinds of cognition, and they none the less recognise the distinction between perceptions and the other sorts of cognition. Hence even if direct awareness as a matter of fact only happens in perception, there is some reason to believe that there must be other marks that distinguish perceptions from judgments. Supposing that there is direct awareness in all forms of cognition, I think we may still be able to find certain common qualities that objects must have for the direct awareness of them to be called perceptions. To begin with, I think there is one negative thing that we can say about perception. Granted that there is direct awareness in all kinds of cognition, still the object of the awareness in the other kinds stands in a peculiar relation to something else, whilst the object of direct awareness in perception does not. When I think of the 47th proposition of the first book of Euclid it is extremely difficult to say of what it is that I am directly aware; it probably differs from person to person, and in one person from moment to moment. But it always has a peculiar relation to the squares on the sides of a right-angled triangle and to their metrical properties such that the whole cognition is said to be 'about' these properties. Now in perception pure and simple there seems to be nothing to correspond to this 'about.' The stage of pure perception of a coloured surface corresponds, it would seem, to the state of a man who just reads the enumeration or sees the figure of the 47th proposition, but to whom the words and figure convey no meaning. Now neither in the case of sense perception nor in the mere apprehension of words are we *forced* to remain at this point, or do we generally remain there. We generally do proceed to other cognitive acts that introduce the notion of 'about,' but perception differs widely from other kinds of cognition in the acts to which it commonly gives rise. The peculiarity of perception is (*a*) that it may give rise to cognitions that are about its own object; and (*b*) that it is not merely held to give rise to them as a matter of psychological fact, but is also the sole and sufficient guarantee of their truth. The cognitions to which a direct perception gives rise and which are 'about' its object are held to be indubitably true. This not purely psychological criterion is, I should say, a sufficient one for a perception. Such propositions have a certainty even greater than that of memory. We recognise in the abstract that memory may be wrong in particular cases, and the nature of the certainty here is that it is reasonable to regard it as right in any particular case until you can prove that it is wrong, whilst any disproof in a particular case will only be possible by means of some other memories, and therefore on the general assumption that memory is true. But we do not even contemplate it as possible that the judgments about the object of a perception to which that perception gives rise should be false.

The matter will probably become clearer if I give some examples of what I mean. Suppose I assert the proposition that Socrates is mortal. I shall undoubtedly be directly aware of something, but, whatever else it may be, it will not be Socrates. If I happen to be a visualiser probably one of the things of which I shall be directly aware when I make the assertion will be an image which is rather like busts of Socrates that I have seen. But no one supposes either that I am saying that this image is mortal, or that my immediate awareness of that image is any justification for the belief that Socrates is mortal. But suppose I judge that my mental image has a snub-nose. This is a judgment 'about' the object of which I am directly aware, and its truth is justified by it in such a way that it would be madness to doubt it. My immediate awareness of the image then was a perception on our definition. Now it is, of course, true that perceptions are commonly held to justify propositions about other things than their own objects, but we must note (*a*) that it is always by a process of inference or comparison that they do so; (*b*) that these inferences and comparisons will never have that infallible certainty which the propositions that perceptions justify about their own objects possess; and (*c*) if it really be a case of perception there will always be some propositions of this latter kind which can be asserted, even though they will not be the whole of the propositions that the perception is held to justify.

Of course it ought to be noted that as soon as such propositions are put into words they cease to be absolutely infallible. The meaning that is expressed to me by the statement 'my mental image has a snub-nose' is infallible, and everyone would admit that I could hardly be mistaken as to the shape of what I perceive. But when I put the judgment into words it is assumed that I mean by 'snub-nosed' what other people do and that I have rightly subsumed the present case. But this involves comparisons that carry me outside the object of my present perception, and in them I have no infallible guarantee of correctness.

The above discussion is relevant to the question of the perception of figure and extension. The point is that different kinds of objects of perception guarantee propositions about themselves which vary very much in number and importance, and that, in the case of spatial qualities and relations, the judgments are made so instinctively and are so much the most important part of what is commonly lumped together under the name of 'spatial perception' that it is much more difficult here than in any other case to decide what is really the object of perception as such. We can perceive a colour without thinking of any propositions about it, and the number of those that it will justify is small, whilst they are not of great practical importance. But absolutely the reverse is the case with objects of spatial perception.

We can now try to decide what weight ought to be given to the discrepancies between sight and touch. In the first place, the purely perceptual element in these two senses is clearly not the same. When I see a cube, and when I feel corners, edges, and faces, it is obvious that what I perceive in the two cases differs. I see extended colours with boundaries; I feel temperatures and texture which also have a quality that I call extension, and I feel certain discontinuities in these objects which I call boundaries. Of course *when* I have identified tactual and visual extension I call the seen and the felt extensions by the same name, and so too the seen and felt boundaries within which there are not these sudden discontinuities. But the objects perceived by the two senses are never the same, for if they were, the sole difference between seeing and feeling would be that one is done with the eye and the other with the skin. But, it will be said, no doubt there are peculiar features in the objects of each sense, viz. colours in sight and temperatures, texture, etc., in touch, but still there is something that is definitely common to the two, and that is extension and figure, which is what interests us now.

Of course it is clear that there is some analogy, but it is very important and not very easy to be clear as to its exact nature. The analogy seems to be that our sensations of sight whose objects are colours give rise to and justify judgments which analyse out of them the quality of being extended, and of having definite boundaries, and of having spatial relations. Our perceptions of temperature and texture can also have a peculiar quality analysed out of them; they too have objects in which there are discontinuities which constitute boundaries; and they too have certain kinds of relations. But, even so, I do not think that the tactual extension, figures, and relations that can thus be analysed out of the objects of tactual perception can be immediately identified with the visual extensions, figures, and relations that can be analysed out of seen colours. For instance, so far from agreeing with Berkeley that distance cannot be perceived visually, it seems to me quite obvious that it is perceived visually and that it cannot be perceived tactually. Whatever may be the cause or the history of my perception it is surely clear that I actually *perceive* the opposite side of the Great Court of Trinity as further away than the side in which my own rooms are. It may be perfectly true that my judgment as to how far it is from my side may depend on previous experiences of walking across the court. But feelings of accommodation and convergence associated with revived muscular sensations *are* not the perceptions of visual distance which the least introspection shows that we possess. Hence I am not at all confident that the extension and figure and relations analysed from objects of tactual perception can at once be identified with those that are analysed from visually perceived objects. What seems to be more true is that in visual extension we can analyse out elements and relations which form a spatial order of the same type as that which we also find on reflection to be constituted by the relations and elements that we can analyse out of the objects of ~~visual~~ tactual perception. We do not perceive an elaborate spatial order, but when we come to analyse and reflect upon what we perceive both by sight and by touch we are led to construct spatial orders of the same type. Of course in people who are neither blind nor anaesthetic these orders are not constructed independently of each other, and so there are not two similar orders left standing side by side, but one which is supposed to include both. It must be noted that when I talk of 'constructing' a spatial order I do *not* hold, as so many people seem to do, that this implies that the order so reached cannot be that of the real world. There is not the least reason to suppose that the view tacitly assumed in so many philosophical works is true that as soon as you have analysed the steps by which you have come to believe that what is real has the quality or relation *X*, it is perfectly obvious that this belief must be false.

For our purpose we are mainly concerned with the belief that the objects of visual and tactual perceptions are such that reflexion upon the products of an analysis of them leads to the knowledge of a common spatial order. Let us take the cube that I both see and feel. Why do I suppose it to be the same cube, and what precisely do I mean by that statement? In the first place under what conditions do I pronounce that *A*, which I feel, and *B*, which I see, are the same thing? I can see an object *B* and at the same time part of my total object of vision may be my hand touching *B*. I see the hand moving over *B*, and, at the same time I feel the different movements of my joints and muscles and the different feelings of contact at edges, corners, etc. There is a correspondence as my eye follows my hand between the different muscular and joint sensations and the different convergences of the eye and between the sudden changes of direction in the boundary lines that my eye sees and my hand feels. Now we localise colours where we see them, and temperatures, as I have already suggested, are commonly supposed to be the same in the body felt as in the hand that feels them. Hence the temperatures are localised within the same space as the colours although the eye cannot perceive the one nor the hand the other. We thus say: There is *a* cold red cube in a certain place because (*a*) both temperatures and colours and textures are found on analysis to yield qualities and relations which on reflexion are seen to form wholes of the same general type, and (*b*) to each relation and discontinuity that can be discovered with regard to these qualities in the object of touch there is found to be one in the object of sight when the eye perceives the hand to be in contact with the object of visual perception. Of course people do not set up an inductive principle that there will always be this correlation. But there is an association which causes an expectation of such correlation between the deliveries of the two senses, and when, as in the case of the painted picture of a cube in perspective, the correspondence fails, we feel disturbed.

But it is easy to see that these facts taken by themselves yield no reason whatever for rejecting the belief that the objects of both senses continue to exist when they cease to be perceived. It merely tells us that to the geometrical distinctions that we discover in the object of vision there will generally correspond felt distinctions in the object that is felt when the finger is seen to be in contact with the seen object. Accordingly when we see an object with the geometrical distinctions that constitute a cube and then find that we cannot feel any discontinuity when the fingers are visibly in contact with a visible edge or corner we feel surprised. But then, as far as perception goes, there were two different objects. One was coloured and from it could be analysed the distinctions corresponding to faces, edges, and corners. The other was smooth and of some temperature, but it had no felt boundaries. We experienced by touch in fact what we generally do when we see that our fingers are in contact with a single indefinitely extended surface. We could move our fingers along the seen boundaries and yet we felt none of the stretching of the arm that usually accompanies such action, and none of that peculiar contact sensation that generally happens when we touch a sharp discontinuity. Well, it will be said, here is the eye assuring us of a sharp discontinuity and also that the finger is touching it, whilst the finger feels nothing of it. Surely nothing can be sharply discontinuous and perfectly continuous at the same time and place?

We have already argued that we only *perceive* distances from the body with the eye, though we may judge of them by muscular sensation. Hence the fact that in the present case of the painted picture of a cube we see the figure in three dimensions, but do not feel any sensation of stretching the arm in visibly following with the finger the visible edges, that go out from the body is not really a case of synthetic incompatibility. It need only show that sensations of movement are not infallible signs by which to judge of distances in directions seen as going out from the body. Still it is right to remember that this is not merely a question of more or less in regard to the judgment of distance. Sight tells you that the object does extend away from you for some distance, whilst touch leads to the judgment that it does not extend at all in that direction. We might in fact put the argument in this way: the seen change of direction of the surface and the felt continuity are held to be at the same place because most of our visual and tactual experiences can best be interpreted on the assumption that muscular sensation is a sign of visual distance. But in a certain number of cases, like this one of the picture of a cube, if you proceed on the same principle you will be forced to hold that at the same place as judged both by muscular and visual tests the surface bends round as judged by sight and remains flat as judged by touch. Now this bending round is not a mere question of judgment. There is an appropriate tactual experience for this kind of discontinuity which is generally found to go together with the visual discontinuity of direction.

Thus the real ground based on synthetic incompatibility of the deliveries of two senses for the rejection of naïf realism is pretty complicated. In the first place you must have identified the objects of the two senses in some way before the argument from the synthetic incompatibility of their deliveries need trouble you. It might be said: either the objects of your two senses are the same or they differ. If they be the same they cannot be incompatible, but if they differ then it does not matter whether they are compatible or not. The way in which the employers of the argument get over the difficulty is as follows. In ninety-nine cases out of a hundred to every distinction of a geometrical character in the seen object it is found that one will correspond in what is felt by a finger visibly in contact with the visible object. Hence, as we have seen, the geometrical qualities that can be analysed out of both sorts of objects are held to be identical and within that common boundary it is held that there exist those other qualities which it is the peculiar function of each sense to enable us to perceive. Hence the objects of sight and of touch when certain conditions are fulfilled are identified. And there is of course so far no reason for departing from naïf realism with regard to the objects of these senses. But now cases arise where this identification breaks down, and where, if we insist on identifying the geometrical qualities that can be analysed out of the objects of visual perception with those that can be analysed out of the objects of tactual perception we shall get to sheer synthetic incompatibilities, such as surfaces at the same place both having edges and going straightly and smoothly on in their old direction. Under these circumstances the first thing to do is clearly to deny the invariable identity of visually and tactually perceived geometrical qualities and relations under circumstances under which they had always before been held to be identical. This however is not by itself any good reason for holding that either the visually perceived qualities or the tactually perceived ones cease to exist when they cease to be perceived; for, as soon as you have ceased to believe that they must be identical, the fact that in a given case they prove not to be so is no objection to either of them. It is quite impossible to prove that the coloured surface may not bend round but that its bent part is intangible, whilst the hard, smooth, cold surface goes on without discontinuity. That is not the view that common-sense takes however. It has found that it never gets a tangible surface without also being able under suitable circumstances to perceive a coloured one, and it rejects as appearance coloured surfaces that are not tangible. There is no objection to such a procedure, but I think we have now seen that the argument from synthetic incompatibility of sight- and touch-deliveries is no *conclusive* reason for doing so, since it is not incompatible with perfectly naïf realism about the objects of sight and of touch.

[1]We may enquire at this point why common-sense should reject as appearance the object of sight and keep that of touch where the identification of the two would lead to synthetic incompatibility.

I think the usual argument that it would put forward in justification of itself would be the following: sight and touch agree in bearing witness to extension and figure, but they differ as to the figure. Also experience has taught us that spatial characteristics perceived by sight will in general have felt characteristics corresponding to them. Now the sole evidence for their existence is that they are perceived. Hence when, as in the present case, it is certain that the spatial relations perceived by two senses differ in part, it is reasonable to hold that those for which we have the witness of two senses are the ones that continue to exist when we cease to perceive them, and that those for which we have only the evidence of one are those that are mere appearances. Now sight and touch agree in bearing witness to the existence of an extended surface, but touch shows no discontinuity in it, whilst sight tells us that it is bent round so as to form a closed surface in three dimensions. Hence it is reasonable to suppose that what is real is that which has both senses for a witness, viz. the flat continuous surface.

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[1] The superiority of touch *after* the Causal Theory of Perception has been assumed is further discussed in Chap. IV. p. 253 *et seq.*

This is the familiar and trivial line of argument which holds that when several senses bear witness to an object it is more likely to be real than when only one does so. It is liable to be confused with the argument that carried Gibbon back from the Roman Church and back into the last will and testament of an affectionate but Protestant father. I want now to point out that it is really very paradoxical and extremely weak if taken by itself.

It is surely clear that the evidence of two senses does not add the least certainty to the existence and qualities of an object of perception of either of them so long as that object continues to be perceived. For that certainty, as we have already seen, is the highest that we can have, and therefore no evidence can hope to increase it. Thus it is equally certain that what I see bends round and forms a closed surface in three dimensions so long as I do see it, whether, as is usually the case, I can feel the corners and edges, or, as is the case with the picture of the cube, I cannot. Hence the argument of common-sense is this rather curious one: although, whilst an object is perceived, agreement of more than one sense does not add in the least to the certainty of propositions about its existence and qualities which each sense separately justifies, yet such agreement does make it more probable that what exists when we cease to perceive it has those properties about which, when it was perceived, the senses agreed.

Now, taken by itself, this is a very odd argument indeed, and I do not think that anyone will find it plausible. It gains any plausibility that it may have from an argument to prove a quite different conclusion. If you are basing a belief in the existence of something that you do not perceive on the fact that if it really exists it will probably produce such and such effects, then the greater the number of such effects observed the more likely it will be that this unperceived object exists and *vice versâ*. Thus Gibbon's argument by which he ceased to believe in the Real Presence was valid enough if we take him to have meant that, if the Body and Blood had been really there, he would have had certain perceptions which he did not have. But the same sort of argument is used to distinguish appearance from reality when the appearance is admittedly there; and here it is quite useless. All that it can prove is that it may not be appropriate to call the object of my perception by the name by which I should if other perceptions were there as well, and not that an object with the qualities that we do perceive with one sense is any less likely to be real than one whose qualities we perceive with two senses. Thus, if the sacramental wine after appropriate manipulation had looked to Gibbon like blood but had tasted like wine, he would not have been justified in thinking it less likely that such a liquid existed when he ceased to perceive it than if it had both looked and tasted like blood. All that he would have been justified in saying would have been: If this liquid which looks like blood and tastes like wine exists when I cease to see and taste it, it will not be safe to call it either blood or wine, since each name will convey wrong impressions to people.

I have discussed here one of the grounds which common-sense would allege for holding that that on which sight and touch agreed was what was real, when it has separated reality and appearance owing to the discrepancy that we have been considering, because I wanted to show the weakness of this common argument. Better reasons for preferring touch to sight will be discussed in the chapter on the Causal Theory of Perception.

We have now examined the argument I (*b*) which attempts to disprove naïf realism from the synthetic incompatibility of the deliveries of two different senses. We have seen that it rests on even more assumptions than I (*a*), and that, like it, it does not by itself furnish any conclusive ground for the denial of naïf realism with regard to both senses. If naïf realism is to be kept however that identification of tactual and visual spatial characteristics which is found to account so well for the facts over a very wide area must be dropped. And, if we separate the two, we shall be left with this remarkable parallelism still on hand and calling for explanation. The actual procedure of common-sense is to hold to the deliveries of touch as real and to make the deliveries of sight, in as far as they conflict with those under circumstances where they might have been expected to agree, appearances. We saw that the argument that what is testified by more than one sense is more likely to be real than what is only testified by one is quite baseless; and we found what precisely such an argument does allow us to infer. So far then we have seen that there is very little reason why common-sense should desert naïf realism just where it does, and no reason as yet why it should make that part of what it perceives real and that part appearance which it is actually found to do.

We pass then to

I (*c*). This is the argument from the incompatible deliveries of the same sense in different people. Here again we are faced by a problem which is partly new but in many ways like that which met us as to what was meant by the identification of the objects of visual and tactual perception. This is the question: What do we mean by two people perceiving the same thing? On the naïvely realistic view from which we start the argument is faced by the old dilemma. Either what *A* and *B* perceive is the same or different. If it be the same there is no problem, if different, then, since the qualities of the objects of their perceptions are qualities of different objects, what matters it that they are incompatible? Why should not both objects exist quite comfortably unperceived by either *A* or *B*?

Another question that arises is this: In what sense is the knowledge that each of us has about the objects of his perception communicable to other people so that we can really know that *A* and *B* (of whom one may be oneself) have at the same time objects of perception which have incompatible qualities? We will discuss these two questions in order. (i) *The Same Object.* I have already pointed out the ambiguity of the word object and it is very important here to bear in mind what has been said about it on pp. 7 *et seq.* What then do we mean by two people perceiving the same object in the sense that the direct object of their perceptions is the same and not merely that something that might be inferred from them is the same? What the present argument wants is that *A* and *B* should be obliged to attribute incompatible qualities to the same reality if they suppose that the objects of their perceptions are real. Since realities cannot have incompatible qualities it will then follow that the object of the perception of one or both of them must be merely an appearance. This however seems to land us in the old dilemma that, if the objects of their perceptions are the same, there is no difficulty; and, if they differ, then by supposing them to be realities we shall merely have two different realities with qualities which, if they came together in the same object would be incompatible—a fact which is not relevant to existing circumstances. This difficulty may be met however by supposing that x and y the two objects are as wholes incompatible with each other and yet as objects of *A*'s and *B*'s perceptions respectively occupy the same space at the same time. Thus the important point is to determine when *A* and *B* hold that the objects that they perceive are in the same place. If they do this, and the objects perceived have incompatible qualities, it will follow that the two objects or at any rate one of them must be appearance, whether they be identical or not. *A* and *B* say that they perceive the same object when (*a*) if *B* takes up the same position as *A* has just had and also a like attitude to *A*'s, what he perceives is indistinguishable from what *A* remembers that he perceived when he occupied this position; and (*b*), as *A* and *B* change their positions and attitudes what they perceive changes in general continuously with their changes of position and attitude; and (*c*) the objects perceived by both and the successive objects perceived by each are localised at the same position of space. Assuming that *A* and *B* can be aware of these three sets of facts then they will say that they perceive the same object. By this they really mean in our phraseology that, at any given moment they perceive different objects, but that the differences are mere appearance, and either what is common is reality, or the appearances are data from which we can argue to the existence qualities of a reality which is not as such perceived in any position of either. But, as we have already said, the really vital point is that the successive objects should be localised at the same point of space and that *A* and *B* can know that their contemporary objects of perception which conform to the rules (*a*) and (*b*) are also localised at the same position. We have yet to discuss how this is possible. Assuming this to be possible we can see where the synthetic incompatibility comes in. Let us take the case of a tangible sphere. *A* and *B*, standing in various places and turning in various ways, will see portions of ellipsoidal surfaces of slightly different eccentricities and rather different distributions of colour. All the conditions that I mentioned above will be fulfilled in this case if they ever are fulfilled. Suppose *A* can inform *B* that he locates the centre of *his* ellipsoid at the same point as *B* does that of his. Then, if the ellipsoids that *A* and *B* perceive be both realities, there will be—independently of whether they be perceived or not—two ellipsoids of rather different eccentricities and distributions of colour having the same centre. Hence we should get the result that not only is neither of these ellipsoids tangible—for all that can be felt is a sphere—but also that each is invisible to one of the observers. This is of course not by itself fatal, since the sphere, which common-sense generally holds to be the reality, is not visible to anyone and never can be. But if we are going to introduce invisible reals at all we shall have to do it on a very lavish scale indeed. We must end by holding nothing less than that there are as many real ellipsoids as can be seen by *A* or *B* from any position and that each is visible from one position and one only (or, remembering that we only perceive parts of ellipsoids and counting parts of ellipsoids of the same eccentricity as the same, from one set of positions and one only). This argument is of course not conclusive. The real world might contain all this host of real objects which are intangible and only visible from certain definite positions. But this is not a view that commends itself to most people, and, if naïf realism involves it, they will prefer to leave naïf realism. There is not, so far as I can see, any real difficulty from synthetic incompatibility on the assumption that all these ellipsoids do coexist and have their centres at one point. It will involve the additional assumption that the presence of parts of the surface of one of them does not render the parts of the surface of others that are inside it invisible from the appropriate position. The argument then does not in the end rest on the alleged synthetic incompatibility but on the terrible complications of the naïvely realistic hypothesis. It therefore gives no disproof on naïf realism, but merely a very strong incentive to seek another and simpler explanation of the facts. That explanation and its consequences, which are far-reaching, is discussed in a later chapter on the Causal Theory of Perception.

But before we leave I (*c*) we must discuss the remaining question: (ii) *Communicability of Perceived Qualities*. For we have assumed all along that *A* and *B* can know that the objects of their perceptions are like each other and that they are in the same place. We must now ask: How precisely is this possible? On the view of naïf realism or of phenomenalism there is of course no difficulty in two people being aware of the same object at the same moment. For it merely involves that *X* shall be at the same time a constituent of two awarenesses p_x and q_x, one of which is a part of the complex of related awarenesses that constitutes *A*'s mind and the other of which is a part of the complex which is *B*'s mind. But the present problem is different. It is: Granted that *A* and *B* can as a matter of fact be aware of the same object at the same moment, can *A* know that *B* is aware of the same object as himself? or, more generally; Can *A* know what are the qualities of the object of *B*'s awareness? Of course we always assume that we can know facts of this kind with more or less accuracy, and our present task is to find the presuppositions of that opinion.

In the first place the most usual way for us to find out what *B* perceives is for him to tell us. Assuming that he is not lying a comparison of what he says he perceives with what we know ourselves to perceive will tell us how like are the objects of our respective perceptions. But this simple method is a good deal more complicated than it seems. Suppose *B* says: 'What I perceive is red,' what information exactly does he convey? What I directly perceive is a mere noise. It is however of course a noise with meaning. When I make noises of that kind I mean to convey information as to what I perceive, and presumably *B* does also. The first thing then is to recognise that the noise that *B* made is the same sort of noise as I should have made had I wished *B* to understand that the object that I perceive has this indefinable quality called red. But why do I suppose that hearing that noise will make *B* think of the quality that I perceive?

Because *B* has been taught English; it will be said. But how was he taught English in this matter? Presumably his parents or guardians in his youth showed him a number of objects which they believed to agree in being red, and made the noise in question. The objects of course all agreed in other respects, e.g. in that of being extended; but they got over this difficulty by showing him a number of other things which agreed with the red ones in being extended and differed from them in colour, and by then making a different noise. Now what were the presuppositions of this method of teaching? In the first place his teachers thought that if *B* were put in an appropriate position he would perceive the same or nearly the same distinctions as they did. They thought that, when they perceived differences, he would not perceive identity, and, when they perceived identity that he would not perceive difference. If *B* had perceived no difference in objects in some of which his teachers perceived one quality (say green) and in others of which they perceived another quality (say red) *B* would use the words 'red' or 'green' indifferently for objects which they considered red and for those which they held to be green. If, on the other hand, his teachers saw no difference in the colours of objects in which he did see a difference, *B* would have argued: 'Evidently "red" is a general name for two different colours'; and he would think it odd that his teachers should have no different names for its two species.

This might indeed land *B* in difficulties when he came to reflect. Suppose he perceives colour differences where his friends perceive none. Call the two colours that he perceives x and y, and suppose that his friends, seeing no difference between the two, call both x. Then, as we saw *B* will think that x is a general term covering both x and y. But then it is very likely that he will see nothing in common between x and y except that they are both colours. In that case the term x will stand in his mind for colour in general. But colour is an universal that extends to other things beside x and y, things that his friends can distinguish. It will then seem very strange to *B* that they only apply the name x to x and y and not to the other colours.

This may lead to the very odd result that, precisely because *B* can distinguish more colours than his friends, he may be led to think that they can distinguish something that he cannot, viz. an universal that is common to *x* and *y* but not to all colours.

Still, in general, *B* could find out and convey to his friends that he perceived a difference of colour where they perceived none. But he could give them no information whatever about the new colour which they cannot perceive except that it is a colour. Thus what seems to be communicable with regard to perceptions is whether one person can always find a difference and a likeness in what he perceives where the other perceives differences or likenesses. A man cannot be certain that the object of his perception is identical with what another person perceives at the same time; but he can be sure whether he can or cannot find a distinction within his object of perception when the other person finds one in *his*. Suppose for instance that an angel could see that when I use the word 'green' another man always perceives what I do when I use the word 'red' and *vice versâ*. Still neither I nor the other man could ever find this out, for we should always agree in our use of the words 'red' and 'green.' We cannot then communicate the objects of our perceptions to each other, and what we mean by saying that *A* and *B* perceive the same object is that *A* can discover no distinction in the object that he perceives to which *B* cannot discover a corresponding one in his own object.

I have used the words distinctions and agreements; and it will be well to consider them for a moment. When *A* and *B* agree in discovering an agreement in their respective objects of perception, we can never be sure that the agreements that they discover are identical with each other. Thus *A* and *B* may agree in discovering the universal 'colour,' i.e. they will always apply the term 'coloured' to the same things, but we cannot be at all sure that, when they see the things about which they agree in finding an universal the universals that they find are qualitatively identical with each other. Still it is perfectly unimportant whether they are identical or not so long as they have precisely the same extension. Moreover, with regard to each such universal there can be a further agreement which further determines it. For instance *A* and *B* may agree in discovering the universals 'red' and 'sweet,' and although we can never know that *A*'s red and sweet as universals immediately analysable out of his objects of perception are the same as *B*'s red and sweet which are analysed out of *his*, yet it will still be the case that *A* and *B* will agree in holding that the two universals differ from each other and from numerous others that they agree in the present sense in discovering. Hence the ultimate fact that the qualities that we perceive and the universals that we find in comparing them are incommunicable, which I suppose is what Kant meant by saying that 'intuitions without conceptions are blind' is no hindrance to knowledge.

There remains one more question. How do we come to be able to communicate these differences and agreements? I show a man two surfaces, one red and the other green. Suppose he is colour-blind. Then he and I agree in recognising *an* universal, viz. colour, but I also recognise two species of it, and he does not. How do I find out that he agrees with me in discovering the universal and differs in seeing only one species of it? In the first instance I believe that he finds an universal because he tells me so, and I believe for the same reason that he does not find the difference. This however assumes that he and I agree in what we mean by agreement and difference. But it might be said: You grant that with a normal man it is perfectly possible that what he perceives when you both use the word green is what you perceive when you both use the word red and *vice versâ*, and the difference can never be discovered. How can you be sure then that when you both use the word distinction he does not mean what you do when you both use the word identity? How do you know in fact that when the man says: 'The objects of my perception have something in common,' he does not mean to indicate the same sort of experience as you have when you perceive that red and sweet differ; or, on the other hand, when he says 'I perceive no difference in colour between this and that,' that his experience is not precisely like yours when you experience both red and green? The answer is as follows: Let us take the colour-blind man and suppose that he is being taught to speak as a child. He is put into circumstances under which we perceive red and green and the two names are told to him. His teacher says: 'This (pointing to one) is green, that is red.' This being so, when his teacher says: 'Pick out the green one,' he can of course do so as long as he remembers which one had that name given to it. But suppose that we try the experiment again, and the things are quite alike in all respects except colour, then the only way to distinguish the one called green and the one called red is for the colour-blind man to remember whether it was the third or fourth thing from the left (say) to which the teacher pointed when he used the word. Hence if the articles be mixed up out of his sight he will be just as likely to choose a red one as a green one when asked to choose one of the latter. In fact he will be seen to hesitate hopelessly, trying and failing to remember any characteristic apart from the original position which marks those called green from those called red. His actions are thus precisely like those that we should make if someone showed us two red or two green things exactly alike in all respects and said: 'One is *A* and the other *B*,' and then shuffled them about while we were away, and, when we returned, said: 'Now pick out *A*.' Now we have assumed all along that the other man is a creature whose mind and body work in the main like ours; unless we had this general conviction we should not have been so silly as to try to communicate with him at all. Hence if, under certain circumstances in which we know that we perceive a distinction, we see that he acts in precisely the same way as we do when we do not perceive one, it is perfectly reasonable to suppose that the state of his mind is like that of ours when we do not perceive a distinction. Hence it is owing to the fact that we have reason to believe that, under certain test conditions, the presence or absence of the recognition of a distinction will vitally affect our outward actions in definite ways that we have very strong ground for holding that we can know that, when a man who has been taught to speak in the usual way says that he perceives a difference, something very similar is going on in his mind to what goes on in ours when we use the same phrase. It is the absence of any such effect on outward action in the case of the matter of our sensations that makes it impossible to decide whether we do really perceive the same object when we agree about our use of the names of such objects. If I perceive red, and, whenever I do so *A* always perceives something exactly like what I perceive when I use the word green and *vice versâ*, I can never find this out by a consideration of *A*'s actions and mine under test conditions. For self-knowledge tells me that I do not act in any two different ways when I perceive red and when I perceive green that can be directly connected with the sensuous peculiarities of the two objects of perception as such. And I must assume that the same is the case with *A*. Hence it is only when he calls red or green both what I call red and what I call green that I can find any difference and reasonably come to the conclusion that I perceive something that he cannot.

Thus the assertion that two people perceive the same thing in the same place can never be *shown* to mean that there is a thing really at a certain place to which the minds of two people are simultaneously in direct perceptual relation. We have reason to believe that, as far as concerns the objects of visual perception, they are never even precisely alike in the sense which is communicable at the same time for two different people. All that the statement really means is that there are certain agreements and differences which people agree or disagree in discovering in their objects of perception. It means, to put it more accurately, (*a*) that when *A* stood in the position in his perceptual space in which he judges that *B* now stands and in the same attitude he discovered no distinctions in the object of his perception to which *B* does not now find corresponding ones in the object of *his*. And we saw that agreement or disagreement on this point is capable of being elicited with fair certainty on the assumption that *B* is a being whose mind works in substantially the same way as *A*'s. (*b*) That *A* judges that the object of his present perception is much like that which he perceived when he stood in the same position and attitude as that in which he now judges

B to be standing. And (*c*) that *A* found that as he moved from one position to another there was continuous variation of those qualities of his successive objects of perception that changed at all. This is what is meant when *A* and *B* hold that they 'perceive the same object'; it is all that is communicable; and, as we have now seen, it is perfectly compatible with extreme qualitative difference between the objects of *A*'s and *B*'s perceptions even when each judges that he is in the same position as has been vacated by the other.

I think then that we may hold that this consideration of what is meant by two people 'perceiving the same object' and of one person 'perceiving the same object from different positions' is a serious stumbling-block to naïf realism. Not only is it certain that the immediate objects of their perceptions at the same time are *not* the same and that the objects of the successive perception of each are not the same; but also, a complete agreement between them in all that is communicable is not incompatible with extreme differences in the objects of their perception. This, whilst it prevents an argument from synthetic incompatibility from being valid, makes naïf realism so complicated and artificial that one is almost forced to the theory of a common cause of the perceptions of each person and to the degradation of the objects of most if not of all of these perceptions to the level of appearance. But this alternative needs a separate chapter.

But there remain other alleged arguments against naïf realism which can be discussed independently of a causal view of perception, and to these we must now turn. We begin with the argument that I have called (III). This deals with the alleged disproof of naïf realism based on the happening of dreams and hallucinations. What we perceive in dreams has most of the qualities of what we perceive in waking-life. On the other hand it is not supposed to be real. Hence the naïf realist is told that this fact is enough to disprove his contention that the objects of perception exist as such whether they are perceived or not. Since to be real, for us, just means to be capable of existing independently of being perceived, the question turns upon what justification there is for believing that the dream world as such only exists when it is perceived.

It is often rejected from reality on the ground of internal incoherence. But I do not think that this position can be maintained. The inconsistency that is noticed in dreams is not *internal* inconsistency, but merely lack of coherence with what happens in waking-life, and this is not really relevant unless you assume that, for the dream-world to be real at all, it must be part of the waking-world. When the Lord Chancellor in 'Iolanthe' suffered from nightmare his dream seems to us to be wholly incoherent because, in it, he crossed the Channel from Harwich in a vehicle slightly larger than a bathing-machine which was boarded at Sloane Square and South Kensington stations by a party of friends to whom he supplied a collation consisting of cold meat and penny ices. Now it is perfectly true that this is a concatenation of circumstances which it is most unlikely for any Law Lord in waking-life to experience, but that does not make it intrinsically absurd. Hence, if he intended to convey that the Channel, the friends, and the stations are those which he respectively crosses, meets, and uses in daily life, we should be justified in thinking (even though a respect for the English legal system prevented us from saying) that he was mistaken. But this by itself is not any good ground for denying that, in another world which he experiences when asleep and which continues to exist when he wakes and ceases to perceive it, there are objects sufficiently like the Channel, his friends, and the stations on the Underground between Victoria and Glo'ster Road to be mistaken for them, which stand in such relations to each other as it would be unlikely or impossible for the things which resemble them in waking-life to stand in. The only argument against this suggestion is that it is unlikely that, within each of two sets of objects corresponding one to one by likeness so close as to lead to mistakes of one for the other, there should yet be two such very different sets of relations. And this is certainly not a strong argument taken by itself.

The next argument against the reality of dream-objects is from the discontinuity of dreams. Taking this objection merely in the form that there are intervals between our dreams in which we experience a quite different set of objects this would of course cut both ways. But the argument is not so weak as this. It says that when dreams interrupt the course of our waking-life we can easily pick up the thread of it again. What we experienced before we slept is continuous with what we experience when we wake once more. But what we dreamt before we wake is scarcely ever continuous with what we dream after we go to sleep again. This is of course in general true, though, in certain abnormal cases, it is not and the successive dreams form as continuous a life as the successive waking experiences. A good example of this is discussed by Prof. Flournoy in his book *Des Indes au Planète Mars*. But in taking normal dreaming we must be careful that the continuity in waking-life shall be judged from an analogous standpoint to the discontinuity of the dream-life. The continuity in waking-life is judged in waking-life. But the discontinuity of dream-life is also judged in waking-life, and this makes the comparison hardly fair. In a fair comparison, either I ought in my present dream to judge that its details are not continuous with those of the dream that I had last night, or else I ought to judge in a dream that what I perceived before I went to sleep is continuous with what I perceived at some other time when I was awake. As far as I am aware we never do make such judgments in dreams, and so the comparison never is quite *in pari materiâ.* The fact is that, when we wake after a dream, we do find a discontinuity, and this troubles us so that we have to consider closely for a little while before persuading ourselves that our dream experience really is discontinuous with the present one whilst the latter is continuous with that we experienced before we fell asleep. In the dream however we rarely do have this state of discomfort which makes us reflect. Hence it remains a question whether, if we did reflect in dreams, we might not find them continuous with past dreams. In this case there would not be real discontinuity, but merely a temporary abeyance of our critical and comparative powers. It is not a fact that in one dream we remember others and positively see that there are incompatibilities between them and the present one, but that we simply do not in general remember and compare dreams whilst in the dreaming state. In waking-life we do to some extent remember dreams, and, on comparison, find them much less connected than our interrupted waking experiences; but then our memory of dreams is notoriously very bad and fleeting, and so this does not really prove very much. Hence I think it is fair to conclude that the evidence for the discontinuity in character between our successive dreams is neither strictly comparable with nor nearly so strong as the evidence for the essential continuity of our successive waking experiences.

Suppose however that the evidence were quite as good, I still do not think that it would furnish any valid ground when taken by itself for denying that the objects of our dream-experiences are as likely to be real as those of waking-life. If I go to sleep and my body be moved, without waking me, from my bedroom to a strange field there will be as much discontinuity between the objects perceived before going to sleep and after waking as there is between successive dreams. We should indeed be very puzzled at first, but we should not believe that this proved that the waking-world was unreal. We should consider what explanation our general knowledge of the waking-world rendered probable and should no doubt come to the right conclusion. In the dream, as I have argued, we do not have much memory, our critical faculties are dormant, and we feel no puzzlement that might set us to work in using them. But, supposing that there be a real world with which we become acquainted in sleep, there is no reason whatever why we should always come into cognitive relations with the same parts of it every time that we enter it at all. We do not know enough of its character to have learnt the general laws that may hold in it and, if known, might explain why at different times we do enter into cognitive relations with different parts of it, such as are the rules about the motion of our bodies in the waking-world. But then we never try to find those laws in our dreams, since the philosopher and the scientist rarely remain philosophic or scientific when they are asleep, and so the fact that we do not find them is no proof that they do not exist. If it were our custom to walk in our sleep instead of it being an exception we should find as much discontinuity between successive portions of our waking-life as we now do in our successive dreams; and if it were our custom to seek for the laws of the dream-world while we were in it we might be able to explain its discontinuities as comfortably as we could those in the waking-life of the somnambulist.

I do not think then that the standard arguments from incoherence and discontinuity supply by themselves much reason for denying that the dream-world is real though different from that with which we come in contact in our waking-life.

We pass then to other arguments. It is urged that what people perceive in waking-life under similar circumstances agrees pretty well as a rule, but that this is not so in dreams. To this argument there is more than one answer. (*a*) In hallucinations and visions produced in definite ways there is frequently substantial agreement. The experiences of most persons who have delirium tremens seem to be very similar, so too those produced by certain drugs like Indian hemp. To these agreements ought certainly to be added those of mystics. On the whole there is a remarkable agreement in essentials between the visions of different mystics, as may be gathered from reading a book like the *Varieties of Religious Experience.* The differences come in, as might be expected, in the philosophical and theological interpretations of their experiences that various mystics offer. I understand that the agreement is still more substantial in the case of Indian mystics who undergo a definite and common form of initiation. Of course all the agreements can be explained, hypothetically at any rate, by a causal theory; but we are purposely leaving that aside in the present chapter. What I wish to point out here is that there is another possible explanation, viz. that all people who do certain things do come into perceptual contact with a real common world which those who do not do these things cannot perceive, and that it is a little hard to follow those people who hold it to be certain that the microscopic structure of nuclei is real because if students follow prescribed rules they will perceive it, and yet hold that the fact that the mystics have equally concordant experiences is not merely explained but explained away as far as the reality of the objects of the latter is concerned by pointing to the fact that they have undergone a common course of initiation. But I also quarrel with religious mysticism for a precisely similar inconsistency. How can it argue that the agreement of mystics who have had a common training tends to prove that they perceive a reality and deny that the agreement of the persons who have drunk themselves into delirium tremens points to the reality of pink rats? The mystic's God and the drunkard's pink rats must, at this stage, stand or fall together. (*b*) No doubt though, in normal sleep at any rate, there *is* great disagreement. The disagreement between sleepers and wakers seems to me to prove nothing; it is that between sleepers and sleepers that is important. But the difficulty is: How can you tell whether two sleepers are in the same relevant conditions? We know pretty well what conditions are relevant to what is perceived in waking-life, but there is no reason to suppose that they would be the same with regard to the world which we experience in dreams. It is perfectly absurd to argue that, because two people in the same bed experience much the same objects when both are awake, they ought to do so when both are asleep if there be a real dream-world. To argue so would be as reasonable as to argue that, if two people happened to dream that they were in the Great Court at the same time, the fact that, when they woke up, they found themselves in their respective bedrooms throws doubt on the reality of the waking-world.

I do not think then that agreement and disagreement taken by themselves are any good argument against the reality of the world of dreams, of delirium, and of mystical vision.

[1]The sole remaining argument with which I am acquainted involves causality and must be deferred. This argument in its crude form would hold that for anything to be real it must be perceived by means of the appropriate organs. Now we see things in sleep with our eyes shut, and therefore it is argued that what we see in dreams cannot be real. Here I have only to indicate the following points. (*a*) The causal argument commonly ends by proving that there is no reason to suppose that what we *do* perceive by means of our organs is real. Hence it can hardly prove that the dream-world is less than that of waking-life. It can only prove that both are unreal and that the perception of both is presumably caused by something that is real. (*b*) It is a common premise of the causal argument that we perceive everything by means of organs of perception. This, as stated, is simply false in view of dream-perceptions, and so it is hardly possible to use *both* the argument from relativity to an organ and that from occurrence in dreams to disprove naïf realism, as Bradley quite happily does. (*c*) On the other hand the causal theory, here as in most places, is capable of accounting for the relevant facts. It accounts for the respective agreements of the drunkards, the microscopists, and the mystics. But what the common-sense and scientific supporters of it fail to see

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[1] Cf. Chap. IV. p. 271 *et seq.* where this question is finally discussed.

is that, *primâ facie*, at any rate, whilst it reduces God to the level of the pink rats as regards reality, it degrades the cell-nuclei to the same position. Now most scientists could bear with fortitude the reduction of the Deity to the level of the rodents; but they resent such an affront to really important things like nuclei.

It remains now to discuss the argument

(IV). This attempts to refute naïf realism by pointing to an alleged close approximation of objects of sense-perception to feelings, and the general intimate connexion between the two. This argument is evidently thought to be a strong one. It is used by Berkeley and not despised by Bradley. One of the many humours of philosophy is that it is also used by Locke to refute the solipsist, a person whom that philosopher treats very much *de haut en bas*. Whilst Philonous tells Hylas that a great heat is a great pain, and proceeds to argue from this premise that temperature is purely subjective, Locke orders the solipsist to put his hand in the fire—a method of refutation that recalls the 'coxcombs'[1] and Dr Johnson with their 'grinning' and kicking arguments for realism.

I will quote Bradley lest we seem to be arguing with thin air. He says: '...The pleasant and disgusting which we boldly locate in the object, how can they be there? Is a thing delightful or sickening really and in itself? Am, even I the constant owner of these wandering adjectives?'[2] Do we 'boldly locate' the pleasant and the disgusting in the object? In general we have seen that it is not of importance to the reality of a quality where we happen to locate it; it is not until we begin to locate incompatible qualities in the same place that difficulties arise. Now, if we locate these feelings at all, we certainly do not locate them at the same time in the same place, so that this cannot be an argument from synthetic incompatibility. Moreover, we saw in discussing sound that we often do *talk* as if we located qualities in what as a matter of fact we regard merely as the cause of the perception of them, although, when we come to consider the matter, we see that, if they are to be located at all, they must occupy a different place from that in which verbally we put them. I suppose then that the best way of stating the argument would be as follows: We do often say that objects are pleasant or disgusting, and yet we are sure that these are not real qualities. Ought not this to make us doubt whether other qualities that we attribute to objects and recognise by sense really belong to those objects?

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[1] 'And coxcombs vanquish Berkeley with a grin.'

[2] *Appearance and Reality* (Second Edition), p. 12.

We must distinguish between the questions of whether a quality be real and whether it really belongs to the object to which it is ascribed. The first simply asks: Does the quality exist unchanged when no one perceives it? The second asks: No matter whether the quality be real or apparent, ought we to localise it where we do? Now the present argument assumes that we know that, with regard to feelings of pleasure and disgust, we can answer both questions in the negative. Pleasures and pains are not believed to exist when not felt, and they are not believed to be localised in external bodies when we come to reflect. And the argument is: If we can know this on reflection about pleasantness which we yet often treat as a real quality of objects, may we not suspect that, if we reflected more carefully, we should see that we make the same mistake about colours and spatial qualities?

To this the right answer seems to be that the more carefully we reflect the less likely we are to confuse feelings like pleasure and pain with qualities like red and square. For, when we reflect, we see that red and square are objects of certain awarenesses but not qualities of any awareness; since we are persuaded that no awareness is red or square. On the other hand, when we reflect on pleasure and pain, we see that, whilst they *may* be the objects of the same sort of awarenesses as those of which red and square are objects, yet in general they exist in an entirely different way in which red and square never do exist. And that is as qualities of awarenesses which are themselves not awarenesses of pleasure and pain, but of objects like red and square. If we go too near a fire we may say that the fire is white-hot and the sensation painful; we never call a sensation white-hot or a fire painful unless, in the former case, we confuse a sensation as a mental event with the object that is cognised in it, and, in the latter, mean that the fire causes pain but does not feel it. But, although we have seen that we never really do mean that a material object is pleasant or painful and that we do seem to mean that pleasure and pain are qualities of the cognitions of those objects which we term pleasant or painful, can this latter view be maintained as it stands?

Do we really mean that the feeling of pain is a quality of our sensation of heat in the same way as the red colour is a quality of the fire? The difficulty is that in the latter case we know pretty well what we mean by x being a quality of Y, viz. that x is located in the same place as the other qualities which together with it constitute Y[1]. But when we call the feeling of pain a quality of a sensation we cannot mean anything like this, for there is no reason to suppose that a sensation is in space. And there is a further difficulty. I have tried to maintain that, in ordinary perception, that of which we are directly aware is full of distinctions which are only made explicit in what are at any rate rudimentary judgments. With regard to the more salient features these judgments are made at once and automatically, but, when there is little interest or the distinctions are hard to discover, the judgments are not in general made. Yet somehow all the distinctions that we can discover by paying attention to our object of perception must have been in it all along, and indeed they are felt in a vague way, whether we distinguish them and discover their relations or not. Now this vague feeling of distinctions and relations that are afterwards made explicit by judgments seems to me to be closely analogous to the way in which we become aware of pleasure and pain. But here arises a difficulty. Just as we can make judgments and find that the vaguely felt distinctions and relations become explicit as the qualities of objects perceived, so we can make our pleasures and pains the objects of cognitions. It would seem that by analogy they ought then to be clearly recognised as qualities of what was previously but vaguely felt, and that the evidence for the view that they are qualities of mental states should be that, on reflexion, they always do turn out to be such. But the trouble is that this is not always the case. Some seem to be as clearly recognised as qualities of parts of the body as any ordinary sense-qualities can be. Hence the evidence for supposing that pleasures and pains differ from sense-qualities in being qualities of our mental states breaks down. Bodily pains like toothache,

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[1] Cf. Chap. II. p. 91 *et seq.* where this view is more fully discussed.

Pleasure & pain not qual. of mental states

when they pass out of the stage of mere feeling and come to be cognised, seem to be as clearly localised as red and temperature. And it is hard to see how they can be at once qualities of the body and of mental states. Nor can it be maintained that toothache is merely a quality of the perception of one's tooth which gets its spatial characteristics by a kind of subreption from those of the object of this perception. For we do not generally have any perception of the tooth at all until the pain begins. It is only then that we put our tongue to it, and it is the previous localisation of the pain that tells us where to put our tongue.

The upshot of our discussion is that (*a*) no sense-qualities are qualities of mental events; (*b*) though some pains may be so it is difficult to tell precisely what such a statement means, and it is certain that this is not the case with bodily pains. Some pains, like toothache, seem indistinguishable from sense-qualities, and it seems to be a strictly comparable use of terms to say: 'My tooth is painful' and to say 'My tooth is black and has a hole in it.'

[illegible]

It is evident, that only bodily pains and pleasures are sufficiently like sense-qualities, for a belief in their non-existence, when unperceived, to be any strong argument by analogy for the unreality of sense-qualities. Hence the question turns on whether we are justified in believing that bodily pleasures and pains necessarily cease to exist when they cease to be felt.

But even before we discuss this question we can see that Bradley's argument is greatly weakened. Pleasantness and disgustingness are not localised in objects, but only the power to produce pleasures and pains. And the pleasures and pains, if localised at all, are localised in our own bodies. It is quite useless to say that it is inconsistent to replace pleasantness by a cause and to keep redness as a quality of a foreign body. You cannot help perceiving red as extended and at a definite place in space, and the close analogy between bodily pleasures and pains and sense-qualities is not an analogy between pleasantness and painfulness and sense-qualities.

However, it will be worth while to discuss whether bodily pleasures and pains be real or only exist when they are perceived. We might either (a) be immediately certain of this, or (b) be certain of it on grounds that depend on the differences between pleasures and pains and sense-qualities, or (c) on grounds that apply equally to both. It is evident that if our certainty springs from (b) or (c) the argument based on it is open to the charge of being an *ignoratio elenchi* in the one case or a pleonasm in the other. If we only assure ourselves that toothache, e.g., ceases to exist when we cease to perceive it on the ground of the *differences* between toothache and sensible qualities, it is clear that no analogical argument from a premise so proved will give you any reason for supposing that sensible qualities are mere appearances. On the other hand, if it be the characteristics that toothache and sensible qualities have in common that make us think that toothache only exists when perceived, the characteristics of sensible qualities will equally dispose of *their* reality without any need to drag in the mention of bodily pains. Hence it can only be valid and useful to argue that sensible qualities are appearances on the ground that toothache only exists when we are aware of it, provided our belief in the latter proposition is founded on no grounds at all. Now, I quite grant that most people believe that bodily pleasures and pains only exist when someone is aware of them. The question is: What, if any, are their grounds for that belief?

The usual grounds that are given are ① relativity to an organ, and ② different tastes of different people at the same time and of the ③ same people at different times. But, as we have seen, these arguments are also brought independently against the reality of sense-qualities, and therefore if they be valid of pleasure and pain and these be like enough to sense-qualities for an analogical argument from one to the other to hold, it would still be a ridiculously roundabout way of degrading sense-qualities to the rank of appearances. To use the argument by analogy under such circumstances would merely be to use a weaker argument where a stronger one is available and has to be assumed.

I fancy, however, that most people would say that it is immediately certain that a pain or pleasure cannot exist unfelt, that its whole essence is to be felt, and that no meaning can be attached to an unfelt pleasure or pain. Now, if this be really an immediate certainty, and if sense-qualities be really like enough to pleasures and pains to make an analogical argument from one to the other plausible, it would be a most extraordinary thing if this immediate certainty should only extend to pleasures and pains and not to sense-qualities. Yet it seems quite certain that it does not extend to sense-qualities, since it is always held to be a paradox that needs support by arguments when the latter are held to be mere appearances. But is this quite so certain as it seems at first sight? I think we shall find that it is not. It is indeed felt to be extremely paradoxical to deny the reality of figure and motion, and rather paradoxical to deny that of colour. But as we go lower down in the scale of the senses, by which I mean as we pass to those whose deliveries seem to be less and less capable of scientific elaboration, the paradox of denying the reality of their objects decreases rapidly. We seem to have little difficulty in thinking of an unperceived shape or motion or colour; but what exactly is an unperceived smell or taste? Would not common-sense think it almost as paradoxical to hold that a lump of sugar has a sweet taste when no one is tasting it as to hold that it has not a cubical figure when no one is seeing or touching it? No doubt the perception of a sweet taste is *formally* just as much analysable into content and object as that of a triangular surface; but, whereas common-sense is certain that in the latter case the object is also existentially separable from the content, it is pretty sure that in the former such independence is impossible. This example brings us in touch with an argument of Berkeley's which, though stated in terms of his erroneous theory about universals, is not essentially dependent on it. May not the belief in the possibility of the separate existence of objects apart from content be the result of a vicious abstraction? It is not a vicious abstraction (as Berkeley would probably have held) to recognise the two elements in any perception, but it is certainly arguable that to believe in the possibility of their separate existence is like believing in the possibility of a separate existence of colours and visible shapes because we can recognise the distinction between the colour and the shape of what we see. It is certainly important to notice two points that favour this view: (1) It is extremely difficult (if it be really possible at all) to become aware of the content element in the supposed complex; and (2) it is commonly believed that, whilst many objects can exist apart from contents, no contents can exist apart from objects.

I think the difference in our attitude towards the reality of shapes and colours and of smells and tastes is expressed in a distinction in popular language. Consider the phrases: That patch looks red; that patch is red; that substance tastes sweet; that substance is sweet. The first two phrases have as a rule distinctly different shades of meaning. When I say 'that patch *looks* red' I generally convey a suspicion that red is not really the colour that the patch *is*. But to say that a thing *tastes* sweet would generally be regarded as equivalent to saying that it *is* sweet. This linguistic distinction is no doubt a mark of our belief that there are real colours and our doubt whether there are real tastes.

Granted, then, that there is a difference of attitude, is there any justification for it? Berkeley's argument would be equally valid as it stands against the reality of shapes and motions as against that of colours, and we defer its treatment to a later chapter[1]. But common-sense makes a distinction, and the question here is whether it has a right to do so. What it claims is that by direct inspection of smells and tastes it can see that they are not the kind of things that could exist unperceived. What, it asks, could a taste that no one tasted be? Such a position implies either that common-sense is in possession of some general principle by which it can assert that things with the qualities x cannot be real, whilst things with the qualities y can be, and that tastes have the qualities x and shapes the qualities y; or else that there are immediate judgments about various classes of objects which assert that the objects of one class are real and that those of another are not. The difference is of some importance. In the first case we should say: Tastes are not real *because* they have the qualities x, and shapes are real *because* they have the qualities y. In the second we should have to be content with: Tastes (etc.) are unreal and do as a matter of fact all have the qualities x; shapes (etc.) are real and do as a matter of fact have the qualities y. I think it will be admitted that if the second be the actual position of common-sense it is an unsatisfactory one, since it seems arbitrary and insusceptible of further argument or application.

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[1] Chapter III.

Let us consider the most important distinctions between tastes or smells and shapes or colours. They are the following: (i) shapes and colours are perceptible at points in visual space that differ from those where the percipient organ (the eye) is present. Tastes are only perceived on the tongue, whilst smells, in as far as they are localised directly at all, are placed vaguely at the nose. (The statement that a body has such and such a smell is admittedly an inference. It means that we smell the air in our nostrils and that we have reason to believe that its smell is due to odorous particles from a certain body—or, as we put it, 'the smell *comes from* this body.') (ii) Colours often have definite shapes, positions, and relations in visual space. Tastes and smells only have a very vague extension without definite shape. If tastes have any spatial relations they are not directly perceived, but reached by correlating them with points of tactual space on the tongue. (iii) Tastes and smells never seem to appear as terms in any causal laws in the physical world. Tastes do not cause other tastes except in the sense that they may influence by contrast, whilst they are never causally connected, as far as we know, with motions or colours. On the other hand, most causal laws are in terms of shapes and motions and spatial relations reached by reflecting on visual and tactual percepts. Of course in this respect colours resemble tastes and smells rather than shapes.

These seem to be the most important differences between the two kinds of objects. Do they provide any ground for supposing that one kind is more likely to be real than the other? I am inclined to think that they provide at least a good excuse if not a very strong reason for the difference of attitude taken by common-sense. Common-sense says that it finds it almost impossible to conceive of an unsmelled smell or an untasted taste, but finds little difficulty in an unseen colour and none in an unseen figure. And the cause of the difficulty seems to me to be that a taste or a smell has so few qualities and stands in so few relations that it approaches that pure being which Hegelians tell us differs in no way from nothing. There are hardly any propositions that can be asserted about tastes and smells. They *are* of course determinate things with a unique nature which makes a smell a smell and a taste a taste. But apart from this sensuous uniqueness there is hardly any thing that can be said about them. They can be very roughly localised in the perceptual spaces of other senses, they are vaguely extended, they can be compared in a slight measure in intensity, and they have ill-defined qualitative differences comparable (though on an indefinitely lower scale) with colour differences. Now, when you try to conceive what an object that you perceive would be if you no longer perceived it you can only do this by thinking of propositions which might still be true about it. And we see that there are hardly any such propositions that can be at all definitely formulated. You cannot say much more than that a smell will still be the smell that it was when you smelled it; but you cannot describe this characteristic to yourself or other people in any general terms.

When, as in shape and motion, and even to a less degree in colour, you have a mass of relations and qualities giving rise to numberless propositions you can conceive where you no longer perceive. For although the qualities and relations were given in perception and still have the sensuous peculiarities which make, e.g., before and after in time something different from before and after on a straight line, still they have general logical characteristics that can be grasped by thought without further help from sense.

But although this seems to me to account for the difficulty felt by common-sense in conceiving an unsmelled smell or an untasted taste, I do not think that it provides any argument against the possibility of their being real. As smelled and tasted they have a definite nature and exist, and the fact that they have few qualities and stand in few relations, whilst it makes it difficult for us to conceive their unperceived existence, in no way disproves it. There is no reason why whatever is real should stand in many relations and have many qualities, and much in fact may be real which we cannot intellectually describe.

But however this may be there is one additional characteristic that even bodily pains have in distinction from ordinary sense-data, and that is their relation to volition. All pains as such we desire to remove. We may of course will to endure a pain because we see that its removal would be incompatible with the existence of something that we think very valuable, or would entail a greater pain later. I think, then, we might say that a bodily pain is a sense-quality located in our bodies which we directly desire to remove. This certainly covers all bodily pains; the only question is whether it may not be too wide.

It might be said that, if one were deformed, that would be a sense-quality localised in the body that anyone would desire to remove, and yet it would not be a pain. This is true; but then it is not a sense-quality that we *directly* desire to remove like extremely high temperature of the skin. We desire to remove deformity because we think it ugly, or because we feel that it makes us repulsive to others, or because it prevents us doing many things that we should like to do. We do not desire to remove it directly and for its own sake without reference to anything else. But when I consider any bodily sensation which I call painful I always find a definite quality located in my body and a desire to remove it for its own sake, a desire on which of course I need not, on reflexion, act. I am not persuaded that there is any more in bodily pains than their being sense-qualities located in the body and standing in this peculiar relation to volition. But this is not important for us. The point is that a necessary condition for a bodily pain is that there should be this relation to the will.

Now I suspect that when a man says that he is immediately certain that an unfelt bodily pain cannot exist, what he means is that he is certain that this relation to the will would not exist unless he were aware of the sense-quality. And here I agree with him. An unfelt toothache would not be an ache, because it does not become one till the will is affected, and what the mind does not perceive the will does not ry good.

trouble about. But this does not seem to me to imply that the sensible quality that is located in the tooth when we do feel toothache cannot exist unperceived. It would, then, merely not affect the will, and so not be a pain.

Now consider Berkeley's argument about the increase of temperature ending in pain. There is no sudden discontinuity in what we feel, as the temperature rises; yet at first most people would say that the skin was really warm and would continue to be so even though we did not feel it, whilst, later on, when it becomes painful, most people would say that the pain could not exist except when felt. Even if Berkeley's solution were right there is a paradox in the change in the person's conviction as to the independent existence of what he perceives as the temperature rises. Berkeley's explanation does not account for this, and I think that ours does. As the temperature rises we still continue to have a sensation whose object is a temperature continuous with what was experienced before, though of greater intensive magnitude. But when this intensive magnitude increases beyond a certain point it begins to affect the will, and we desire its removal directly and for its own sake with constantly growing vehemence. Now the subject is certain that nothing can affect his will of which he is not aware, and therefore he says that the pain could not exist if it were not perceived because temperature does not become a pain except by affecting his will in this way. But where he and Berkeley are wrong is in supposing that this proves or renders it probable that temperature as such cannot exist unperceived, contrary to the plain man's belief when the temperature was lower. All that he has a right to conclude is that the temperature could not be painful unless it were perceived, not that it might not exist unperceived.

Thus the argument from bodily pleasures and pains seems to me to have no validity taken by itself. And it is only such feelings that resemble sense-qualities enough to make an analogical argument from them to the case of sense-qualities plausible. I therefore conclude that this argument by itself will not do what it claims. It is largely confused with arguments against naïf realism based on relativity to an organ and disagreement among different subjects. And these can only be fully discussed when we deal with the causal theory of perception.

We have now discussed all the important arguments against naïf realism that do not depend on causality. They have commonly been accepted by philosophers without criticism and handed down from classical scepticism to Descartes, and from Descartes and Locke to modern thought. We have found that some are entirely futile; that none make naïf realism absolutely impossible; but that to maintain it in face of the discrepancies between sight and touch and of the visual perceptions of the same person in different places and of different people at the same time would be to assume an immensely complicated world of largely imperceptible reals for which there is no evidence except the doctrine of naïf realism itself. These complications are swept away by a causal view; but such a view has its own troubles, and these have yet to be discussed. But before doing this we must digress for a chapter and consider the Law of Causation itself. It is vital to science in general and to the causal view of perception in particular, and it has been severely handled by philosophers. To it we must now turn.

CHAPTER II

ON CAUSATION; AND ON THE ARGUMENTS THAT HAVE BEEN USED AGAINST CAUSAL LAWS

IN this chapter we propose to discuss Causation, and more particularly certain problems connected with it on which objections to causation have been based. It is clearly quite useless to discuss the causal theory of perception and its bearing on the reality of what we perceive until we at least know whether there is any reason to believe that causal laws are necessarily invalid. At the same time this chapter will omit some considerations which, though vitally necessary for a complete view of causation, are beyond our present scope. I shall assume in fact the belief that the results of proceeding upon the theory of probability are correct without exhaustively discussing the nature and validity of the axioms on which it is based. This practically means that I shall assume what everybody does assume that the ordinary processes of induction do lead to propositions which are probably true; though we might find it very hard to say on precisely what grounds we believed this.

What then does Causality mean? This question has, of course, been discussed *ad nauseam*, but the discussion has lead to so little agreement that it is worth while to enter upon it again. There are two characteristics about which most people would agree that they are essential to causality: (*a*) It deals with the existent, and (*b*) it deals with it so far as it is regarded as changeable. Unfortunately this happy agreement is somewhat marred by the facts that existence is a very vague term, and that some people do not believe that anything that changes exists. This difference, however, is largely verbal. In the first place, although people cannot define existence and do use the term with some looseness, yet it is possible to give an extensive definition by pointing to the sorts of things that everyone believes to exist. It is still easier to point to the sorts of entities that people agree in believing not to exist, and happily, this is really what concerns us most. People mean by saying that causation only applies to things that exist that it applies, if at all, to what can change; and they believe that, if anything can change, it is things like chairs and tables and minds, and not those like the propositions of Euclid and the multiplication table. What would be agreed then is that, if causal laws apply to anything, it is to what can change in so far as it changes.

Then, however, we have the trouble that some people think that only the changeless is real. They then proceed to identify reality with existence, and so there ceases to be any genuine agreement as to that to which causality applies. But this is an unnecessary mystification. We have already tried to show that all the characteristics that appear must at least qualify appearances and be true of them in order that we may be able to argue from these characteristics of appearances that they are not realities. Hence, even if change be self-contradictory, and therefore all that changes be appearance, yet these things that are condemned as appearances because they change must truly change if the argument against their reality is to be valid. Thus it might be true that what changes can never be more than appearance and that whatever is real must be changeless; but this does not alter the fact that there is a whole world of appearances which truly change. In as far as these appearances and the supposed timeless reality are alike in differing from such entities as the propositions of Euclid and the multiplication table, we can say that both the truly changing appearances and the truly changeless reality exist. And then the person who does and the person who does not consider that change is a mark of appearance will be able to agree that, if there be any causal laws, they apply to that part of the existent that changes in so far as it does change. They will only differ in that the former will have to say that causality only applies to appearances, whilst the latter will not see any reason to doubt that it may also apply to reality. There will remain for both the question: Are there any causal laws, and, if so, what precisely do they claim to tell us?

What is the characteristic of what we agree to call cases of causation? I throw a stone, we will suppose, and it breaks a window. Here we say either that the stone breaks the window or that I do so. And this is said to be a causal action between me or the stone and the window. The first point on which we must be clear is what is to be called the cause. Reflexion would be held to tell us that, although I may be called the cause of breaking the window, this is at best an elliptical way of speaking. It will be said that accurately I am the cause of the motion of the stone, and the stone is the cause of the breaking of the window. But, so put, there is a distinction which is, I think, generally made between the sort of things that are causes and the sort of things that are effects. The stone is a substance, in the sense that it is a subject and cannot be made a predicate. But the breaking of the window is not a substance; it is an attribute of the window. It can be both a subject and apparently a predicate too. Not, however, a predicate in quite the ordinary sense. If we say that a window is red we no doubt generally mean that it is red now, but we do not bring in any explicit reference to time. It may have been red ever since it was a window. On the other hand a window breaking is an event which implies a definite beginning in time; the window cannot always have been breaking. (There are, of course, other differences between a window being red and being broken. The latter state is analysable and the former is not; but these differences are not relevant for our present purpose.)

The effect here then is an event, but it is also an event that, as we put it, 'happens to' the window. On the other hand, the cause, if taken to be the stone, is not an event, but the sort of thing that has qualities and to which events 'happen.' Are people prepared to maintain this distinction between causes and effects, which is undoubtedly implied in their habitual usage of the terms? I do not see that they can do so. There is always more to be said about the stones that break windows than that they are stones. In fact whilst there are many stones that are moving which do not break windows, there are no stones that break windows that are not moving. Hence, it seems necessary to hold that it is not merely the stone that is the cause of the breaking of the window, but either (*a*) the motion of the stone, or (*b*) the moving stone. Either the cause is a substance qualified by the fact of a certain event happening to it, or it is the event taken as happening in a certain substance. The effect, so far as I can see, would always be said to be an event happening to a certain substance and not a substance qualified by the occurrence of a certain event in it. No one would say that the effect was a window in the act of breaking in the same sense as he might say that the cause was a stone in the act of moving. For this would seem to imply that the cause produced the window as well as its state of breaking, which nobody believes.

Can we decide then, whether the cause ought to be called the moving stone or the motion of the stone? We must first see what exactly is the difference between the two. When we talk of the motion of anything the phrase is ambiguous. We may mean either (*a*) merely the fact that at different moments of time the same thing is at different places, or (*b*) we may mean a supposed quality which all bodies of which (*a*) is true possess so long as (*a*) is true of them. If (*a*) be all that is meant by the motion of a body, then all the actual facts involved about it are its relations to certain moments of time and to certain points of space. It is at one point at one moment and at another point at another moment, and that is all that there is to be said about it. On the other hand, if (*b*) be true it also possesses a quality all the time for any two moments of which it is in different positions; viz., a state of motion which may be greater or less at different times. Now, if causes be particular existents, as is commonly supposed, the only particular existents involved in the motion of the stone are the relations to different points of space and time. Now it might be a true account of causality to say that the cause of the breaking of the window is the stone at-s_1-at-t_1, the stone at-s_2-at-t_2, ...; but I do not think that this is what people suppose themselves to mean when they say that the motion of the stone causes the breaking of the window. I think they mean that, although all these relations exist, they are not the cause of the breakage, but rather that they involve in the stone the existence of an intensive magnitude of some quality which is the real cause.

On the other hand, the people who think that it is the moving stone that breaks the window and not the motion of the stone would say that it seems absurd to make a quality or state the subject of an active verb. They would say that a state of motion no more breaks windows than fair words butter parsnips, that it is only a thing that has this quality or state to which the active verb can be applied, and that the very fact that those who hold the opposite view cannot take their state of motion in the abstract as a cause but must make it the state of motion of the stone gives away their case.

The foregoing discussion ~~as to what~~ common-sense takes to be the cause in interactions which it believes to be causal may seem trivial, but it is really important as introducing us to a duality of view whose further elaboration leads to the conflicting philosophic notions about causality. These two aspects are (1) that of activity, and (2) that of regularity and predictability. Activity may be a very discreditable category and we may ultimately have to reject it, but the fact remains that people do believe until they become sophisticated that there is something more in causal laws than mere uniformities. The activity view leads us to say that a cause is always a thing in a certain state, and its elaboration often leads to idealistic theories of the universe. The uniformity view leads to a theory of causation like that of Mach or of Mr Russell. Since it is uniformity that interests science it is not surprising that those who have approached causation from the side of physics should have laid stress on that side of it. Still we have no right to neglect the other side which is historically more primitive and is certainly believed to be equally essential by common-sense. If it leads us into difficulties we must reject it, but we must not reject it unheard.

We will begin then by attempting to understand what is meant by the activity view. In the first place we must notice that, when the law of causality is stated in general—as distinct from particular causal laws—in the form that every event has a cause, this *may* indeed be meant in the uniformity sense of causation, but probably most of the assent that it gains is received from the activity view. That to which people think they are assenting when they agree that every event has a cause would seem to contain two quite distinct but often confused propositions. One is a law of causation in the activity sense, and one is a law of uniformity; but neither is really the law of causality in the sense that it must have if all causal laws be merely uniformities. People mean (*a*) that they do not think that things that are quiescent suddenly explode into change unless something other than themselves forces them out of their quiescence. If such an explosion has apparently happened you will find, they believe, on investigation either that they were not really quiescent, or that they were not really let alone by other things. And, even if you cannot find evidence for either of these alternatives, they would still say that it was certain that one or other of them had been fulfilled. That a thing genuinely did nothing for five minutes or five seconds and then exploded into change without being acted upon from outside, common-sense refuses to admit, whatever the appearances may be. (*b*) People are also prepared to admit that if *A* forces *B* to have the state *C* at one time it will force it to do so at any other time so long as nothing else is forcing it in another direction on the second occasion which was not acting on the first. But neither of these propositions, which I think must be what common-sense believes when it accepts the general law of causality, are what the uniformity theory of causal laws must mean by this general law. The uniformity theory can only mean by the law of causality (*a*) that there are causal laws; and (*b*) that every event is so connected with other events by one or more such laws that if enough events had been known its happening might have been predicted. Now I do not think that common-sense would hold that the proposition (*b*) here was self-evident in its own right. It would agree to it because it would hold that every event must have been forced to happen by something else and that that something acts uniformly. Thus (*b*) is only admitted by common-sense, because of the proposition, of which it is immediately certain that every event must be forced to happen; without this neither proposition of the pure uniformity theory would be held to be certain *à priori*. Common-sense would say that, if it were offered the uniformity theory alone, it would be prepared to admit that the law of causality as offered would be very useful if true, that clearly a great many events are connected by laws of uniformity which allow of prediction, and that experience proves that the best way to discover such particular uniformities is to take the view that they always can be discovered if we only look carefully enough as a methodological postulate.

Can anything be made of this activity view? Let us take the law of causality first. All that is actually open to investigation in it can be stated in terms of the uniformity view. For all that possibly could be observed on any theory is the changes in various things and their temporal relations to each other. If, then, the activity view were right all that could actually be observed would be that, when a body *A* has not been noticed to change at all for a finite interval and when it suddenly begins to do so, we can always find either that some other body has changed, or that *A* really was changing during the interval when we supposed it to be quiescent. But, taken as it stands, this amount of observation gives a ridiculous law. Of course, whether *A* has changed or not, we shall always be able to find plenty of bodies that have changed during the time in which we have been observing *A*. *If* we know what forced *A* to become *A′* the activity view would admit that we had discovered a causal law, but since we can only decide that it was *X* and not *Y* that acted upon *A* by observing uniformities in *X* and *A*, it is no thanks to the activity view that the causal law has been discovered. Everything that can be observed would have been the same if nothing but the uniformity view were correct. Thus we reach two conclusions about the activity view: (1) it is perfectly useless to science, and (2) no kind of observation of external things and their changes could prove it.

Some people have thought that the observation of our volitions and their effects could prove it. This opinion, however, is ridiculous. No doubt, when I will to move my body I do have a peculiar feeling of effort, and so too when I try to remember something that I have forgotten. But then I am a spirit which has to use a body to exercise its volitions, and it is not at all surprising that the use of my body should give me feelings which I call feelings of effort, since I know on other grounds that the changes in my body are capable of producing feelings in me. If other things that apparently interact causally were spirits, interacted voluntarily, and had to use bodies to do so, there might be some reason to suppose that they had the same sort of feelings. Otherwise there is none. For we differ from billiard balls in precisely that respect which makes it probable that when billiard balls hit each other they do not have feelings of effort.

I suppose that the answer to this will be as follows. No doubt if billiard balls are not conscious they are not conscious of feelings of effort. Still we are conscious, and our feeling is not a *mere* feeling, but is indicative of something which is present in all cases of genuine causation. It is not the feeling that *is* activity, nor does being active just mean that we have this peculiar feeling, but the feeling is an indication of some real quality in us when we exercise causality. This real quality, it will be said, can be assumed to be present in all cases of causality. Over volitional causation, there would be just the same controversy between the two schools as to what precisely is to be called the cause, as there was over what was the cause when the stone breaks the window. The believer in activity would say: I, with this state of volition, am the cause of the motion of my arm. The descriptionist would say: From this particular state of volition the motion of my arm at some later moment can be inferred.

Supposing that my feeling of effort really were an indication of some quality in me when I exercise volition, the question remains: What exactly is that quality, and is it of such a kind that it can be supposed to be present in stones, and billiard balls, and other non-voluntary causal agents? What, in fact, is really meant by transitive verbs? Clearly they imply a relation, though not necessarily to a different term from their subjects. If I say 'I hurt,' the statement is incomplete because we need to know what I hurt. But what I hurt may be myself. In fact, the difference between active and reflexive forms of transitive verbs expresses on the activity theory the difference between transeunt and immanent causation. It appears then, that the same thing can be both active and passive with regard to the same action. But it might be denied that the same thing is ever active and passive at the same time with regard to the same action. I may stick a pin further and further into my body for some seconds, and there will be pain contemporaneously with the motion of the pin. It would seem that in this case I was active and passive at the same time with regard to the same events. But this depends on whether one accepts or rejects the view that causes must always precede their effects in time, which has not yet been discussed. If they must be earlier then the position and qualities of the pin at earlier moments will determine the pain at some later moment, and no moment shall I be active with respect to the cause and passive with respect to *its* effect, though I may be passive with respect to an effect of a previous cause which is very like the one at present operating.

Again, it is an old truism that nothing can be perfectly passive; the way in which it reacts depends as much on its own nature as on that of what is said to be active and is called the cause. The believer in activity might say: No doubt the object in which the effect is being produced is affected differently, according to its nature by the same cause. But this does not make it active in the same sense in which we hold that the cause must be so. For the cause is constraining it to do something which, though definite and determined by its own nature, is yet not what its own nature would have determined had the cause not acted. You could only say that the body affected was active in the same sense as the cause, if it actually modified the cause. Now, in a great many cases, we know that no effect is produced by the object in which the original effect occurs in the object that causes it, and in these cases it is reasonable to say that the object affected is, with respect to the cause, purely passive. Thus, to take Lotze's illustration of the style writing on wax, no doubt if the wax were not soft the style would not write on it, but if the style, unlike the wax, remains unchanged throughout the whole process we can still say that with regard to this causal action, the style is active and the wax passive.

Can this answer be accepted? Let us begin by remembering that, on the present view of causation, the cause is the style in a state of motion. But in what state of motion? Is it the state in which it would have been if it had not touched the wax or anything else, or the state in which it is when it is actually writing on the wax? If the former, then clearly the wax is as active as the style; in fact there is reciprocal causation. The style is supposed to be active because it determines states in the wax which its own nature would not have determined alone. But the wax determines states of motion in the style which *ex hypothesi* it did not have before it touched the wax, and therefore it is active in precisely the same sense as the style. If, on the other hand, we choose to say that the cause of the writing on the wax was the motion of the style as it was actually determined to move when it was in contact with the wax, then, indeed, the wax was not active with respect to the immediate cause of the writing. Still, it was just as active in changing the original motion of the style to that which is now taken to be the immediate cause of the writing. It would seem then, that if a thing is said to be active in so far as its nature determines changes in something else which, had the latter been left to itself, would not have happened, the object in which the effect is produced must in general also be said to be active with respect to the cause.

It is worth while to point out how the above discussion as to the implications of the terms 'active' and 'passive' agrees with what has already been said as to the usual distinction that is made between the sorts of things that are causes and those that are effects. We said that by effects common-sense always means the states of things, but that by causes it never means the states of things alone, but always things in certain states. Common-sense does not say that the motion of a style makes marks on wax, except elliptically, for the statement that the style in motion makes the marks. It does not say that the wax with marks on it is the effect, but that the marks on the wax are the effect. And when we come to discuss activity and passivity it does not say that the marks on the wax are the cause of the change of the style in motion, but that the wax with its marks is the cause in the change of motion of the style. In fact it holds that objects with their states are active or passive, and, as effects are the states of things but are not things, it does not say that effects are passive, but only that the objects whose states are effects are so.

We have now seen that if all causes are active and all objects in which effects are produced are passive the converse also holds. But this is no reason for rejecting the activity view. No doubt if A be active with respect to B, and B with respect to A, it will follow that A is both active and passive with respect to B.

And this may appear impossible, since activity and passivity with respect to the same object appear to be incompatible. But when we examine a particular case we see that there is no particular harm in such couples of propositions. The style is said to be active with respect to the wax, because, if it were not for the style, the latter would not of its own nature change in the way that it does. The wax is at the same time active with regard to the style because the style if not in contact with the wax would have moved differently. At best this could only prove that causal action is always reciprocal, by which it is meant that *A* never affects *B* in any way without *B* affecting *A* in some way (which may, of course, as an unusual particular case, be the same way as that in which *A* affected *B*).

The whole question of activity then, seems to come to this one of a thing forcing another to do that which, if it were left alone, it would not do. The question is: Can anything more be made of this notion of 'forcing' than what the descriptionist makes of it, viz., that there are synthetic laws of the successive states of things in accordance with which, from a knowledge of the states at a certain number of times, those at any time can be determined, and that generally the states of more than one object have to be considered in order to determine those even of one object? To answer this question we need to find out what is meant by what things would or would not do 'of their own nature.' We generally suppose that a thing left to itself will not suddenly go out of existence or come to pieces. But we cannot build very much on this. A shell appears to be a thing with a nature. It does not cease to be a thing with a nature when its fuse is lighted. But after a while it explodes without further external interference. But this is not really a fair example. A shell with a fuse is not one thing, and it is not really quiescent when it seems to be so. It has several parts, one of which is the fuse, and the fuse has been lighted from outside and continues to burn till it produces that effect on the other parts of the shell that causes the disruption. It would seem, therefore, that the thing that is expected to keep quiescent of its own nature for ever must either be perfectly homogeneous or really have been undergoing no change for a finite time.

It is the latter that is the important characteristic. It would be held to be just as surprising for a shell with the fuse unlighted suddenly to explode as for a shell with no fuse at all to do so, providing nothing affected it from the outside. I think, then, that common-sense would deny that homogeneity is essential, and would hold that even a huge system, like the solar system, ought to do nothing if it were once quiescent and even then left to itself. This belief apparently contradicts the law of gravitation. No doubt if the planets were all stopped dead for a finite time and then let go, they would begin to move under their mutual attractions; but then the letting go of them is an external event, and so the belief of common-sense is compatible with the law of gravitation. Thus the first point on which common-sense is certain as to what things can do from their own natures and what they have to be forced to do, might be put as follows: If any system, whether under the influence of other objects or not, has been quiescent for a finite time it can only be through the action of external objects and not through its own nature that it ceases to be quiescent. Such a system will neither change its qualities nor move as a whole or in parts, nor cease to exist as a whole or in parts unless it is forced to do so by changes in objects external to it.

This opinion is closely bound up with the question of the continuity of causation on which it is customary for philosophers to base an antinomy with which we shall later have to deal. The point is that, if what I have said be what common-sense really holds, then it may believe that within any system, so long as there is no moment after which all the qualities and relations of all the parts of it remain unchanged for a finite interval, changes might proceed 'of its own nature' without assistance from outside. But as soon as all changes have ceased during a finite interval however short, they cannot start again except by the action of objects not contained in the system. Now this belief sets a limitation on the kind of causal laws that are possible. It cuts out immanent causal laws in which the only condition is lapse of time. Such a law as: After being quiescent for ten seconds the system *A* will again begin to change is *logically* a possible one, but it would be ruled out of court by the present view.

When we pass from changes from quiescence to merely continuous states of change we find it much harder to decide what a thing or system is supposed to do 'by its own nature,' and what has to be forced on it from outside. The distinction clearly assumes that we can know what a thing's nature is, and what it can do independently of ~~the effects of other things~~ on it. Now this might be possible if we could consider ~~systems which~~ are isolated and are not quiescent, and find out their characteristics. We might then say that the changes that went on in this period were due solely to the nature of the system. But then the only guarantee that you can have that the system is isolated would be the knowledge that, as a matter of fact, nothing outside it had acted upon it during the time of observation. And the only way of judging on this point brings us straight back to the other view of causation. We can be sure that a system is isolated when and only when we find that a selection of data within it at various times will enable us to infer that which we actually can observe at other times in it. Of course, the fact that we cannot do this in a particular case does not prove that the system is not isolated, for we may merely be ignorant of the law of the changes of the system. But until you have actually *found* such a law immanent within the system you have no right to suppose that the changes in that system are expressions of its own nature in the sense required. But even so, the isolation is only known to be relative. Under the supposed conditions we can conclude that the system has not been acted upon from outside while we observed it, but we cannot assume that the section of its history which we observe, and are able to bring under purely immanent causal laws, was not begun by action from outside which happened before we started our observations. In fact, to be perfectly sure that the changes that we observe in a system are wholly the expression of its own nature we should (i) have to discover purely immanent causal laws which would give, as the state at any moment during the time of observation, the state that is actually observed at that moment; and (ii) to be certain that at no moment *ex parte ante* would those laws fail to give what might have been observed. When both conditions (i) and (ii) are satisfied we may say that the changes of a thing or system are purely the expressions of its own nature[1]. It follows, however, from the conditions that it is practically always impossible to be certain that the changes in any system are expressions of its own nature, and, therefore, impossible to tell how much is forced upon it by other things.

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[1] Even so there may be necessary external conditions, and our apparent ability to find a purely immanent law may be merely due to their permanence.

I think, then, we may sum up our discussion of the activity theory of causation as follows. (i) Activity certainly cannot be observed in the external world. We can only observe those regularities from which we infer causal laws in the sense of the uniformity theory. Hence, if you put activity into causation as an essential element it must be something discovered by thought and not based by the ordinary processes of induction on what can be observed. (ii) With regard to the so-called feeling of activity it seems almost certain that it depends on the fact that we are minds and in volition produce states in our bodies which are causally connected with certain feelings in our minds. Hence, if activity be identified with feeling of activity, there can be no reason to believe that it is a general characteristic of *all* causation unless we are already convinced on other grounds of an idealism like that of Leibniz or Lotze or Dr McTaggart, which holds that all substances are minds. (iii) On the other hand, if we take the more promising position that the feeling in the case of agents with minds and organic bodies is not a mere mental event, but the sign of a real quality of activity which exists whether felt or not in all causal agents, we must be prepared to state what we mean by this quality and what can be known about it. We found that it was no objection to it that in many and perhaps in all cases it would have to be found both in the cause and in the effect; because this would merely mean that most or all cases of causation are cases of reciprocal causation, which is a perfectly harmless proposition. (iv) But when we come to ask what activity and passivity mean we are referred to the notion of one thing forcing another to do what it would not do of its own nature. When we came to enquire into this we found that the only way to decide what anything could do of its own nature was by the introduction of causal laws in the uniformity sense of the word. We can express everything that is conveyed by saying that one thing is forced to do something which it would not do by its own nature by the statement that its changes can only be inferred by laws that make use as data of events in other things. And we can state the belief of common-sense that all systems that begin to change after being quiescent for a finite time have to be forced from outside in terms of the rival theory by saying that a law of causation that is immanent to a system can only be found for continuous changes. Thus the notion of forcing things to do what their nature alone would not let them do is perfectly compatible with and explicable by the view of causation that reduces the latter purely to causal uniformities. But this is the only meaning that has been proposed for the terms active and passive, and therefore there seems no reason to suppose that they stand for qualities involved in all cases of causation over and above causal regularities. (v) So far as I can see the only way to retain activity would be to say that it is just an unanalysable quality, like red or green, to the reality of which our feeling when we exercise volition bears witness. With regard to this modest claim we can merely say that there seems little reason to believe that our feelings do attest the existence of such a quality even in the case of causation by volition, that there is still less to believe in it in other cases of causation, and that anyhow the important part of causation has now been shown to lie wholly in the causal regularities.

We shall therefore assume for the future that the essence of causality is causal laws and that these are laws of a certain kind about change. We want in the rest of this chapter to discuss the nature of these laws and the objections which philosophers have brought against them, with a view to seeing how far they can be trusted. The first important question is: How, exactly, are causal laws related to things?

Causal Laws and Things. This question keeps us very close to our old discussion about what things do by their own nature and what they are forced to do by other things. Just as the activity view overrated the importance of substances in causality by saying that all causes are substances in certain states, so the descriptionist view is liable to underrate it by talking as if causal laws merely connected events in general and not events in particular substances. Take for instance the law that successful volition is followed by pleasure. The descriptionist is, no doubt, right in saying that this law means that whenever successful desire occurs it will be followed by pleasure. But what is also important to notice is that the general law only holds when the volition and the pleasure are supposed to be states of the same person, whatever that may mean. Smith's successful desire may very well cause pain in Jones. The notion of cause then is very closely connected with that of things, since it is necessary to state to what things the events that are connected as cause and effect happen.

What then is meant by one thing, independently of causation? This depends on whether the thing in question be a mind or a body. In our case it is more important to consider bodies. With regard to them I think that we are simply forced to deny that there is anything ultimate or recondite in the notion of one thing. There are two different points to note in the definition of one thing: (i) What distinguishes it at a given moment from other things at that moment? and (ii) What makes us call a certain succession of states the states of one thing that persists through time? And the answers that I would suggest to these two questions are (i) Homogeneity at a given moment of sense qualities within a definite boundary in space, and (ii) That the changes that these qualities and this boundary undergo are sensibly continuous. Both these crude statements will need discussion and qualification. We will take them in order.

(i) What precisely do we mean by saying that at a given moment one thing is an aggregate of qualities in various relations and all within one definite boundary which is closed? On this view a state of a thing is a quality which is a member of such an aggregate. The assertion that *P* is a state of a thing *S*, which is generally put in the form *S* is *P*, means that *P* is a member of that aggregate of related qualities within a closed boundary to which we give the name of the thing *S*.

Various objections can be brought against this view. The main ones are (*a*) that qualities themselves have qualities and relations, and clearly *they* are not aggregates of qualities in relation and within a boundary; and (*b*) that the account that I have just given of what is meant by predicating a quality of a thing is not what is meant, but that it is essential to introduce the notion of a substance distinct from the qualities.

(*a*) We do not deny that qualities have qualities and relations and that these can be predicated of them. Nor do we deny that the same *form of words* is used to denote the two sorts of predication; e.g. there is no difference of form between 'this table is red,' and 'this colour is red.' But we shall do well to remember that a great many different meanings are covered by the same word—'is.' For example, by implicitly assuming that 'is' means 'is identical with,' Mr Bradley comes to the most startling conclusions about substantives and adjectives; but surely no one supposes that, because the same form of words is used to express identity and inherence, therefore they must mean the same thing or else nothing at all. In fact, the argument that our account of the predication of qualities of substances does not cover the predication of qualities of qualities comes very ill from the supporters of substance, since they are in precisely the same position. For their theory of the first sort of predication is that it asserts the inherence of a quality in a substance, whilst their theory of substance being that it is that which can be a subject but not a predicate, it is clear that the inherence view cannot also apply to the predication of a quality of a quality.

I do not think then that the fact that qualities can have predications made about them helps the substantial account of the states of things as against ours. What ours requires is merely a further discussion of the nature of the relations between the qualities and of the aggregate of which they are elements in those cases in which the aggregate is said to be a thing with the elements as its qualities.

We said that a closed boundary is *the* essential characteristic of one thing at each moment. As long as there is a perceptible gap between a candle and a candle-stick no one would think of calling them one thing; but, as soon as the candle is stuck into the candlestick it is felt to be reasonable for many purposes to call them one thing. Of course, as it is also known that they can be put apart, and quite frequently are so, it is also reasonable to call this one thing an aggregate

Substance = Subj. not pred.

of two things. But even this is mainly because the old spatial boundaries are still visible. People would say that whiskey in one glass is one thing and water in another glass is another, but, when they are mixed, they would be much more likely to call the whole one thing.

Thus, if our theory be right, one of the main circumstances in constituting one thing of which qualities can be predicated is the possession by those qualities of the quality of extension and their standing in spatial relations. The qualities of one thing at a given moment are extended sense-qualities like colour, temperature, the objects of tactual sensation, etc., which together occupy an extension outside the boundary of which there are either no or very different perceptible qualities for a finite distance. Some of the qualities may occupy the whole extension and more than one can do this provided they be not synthetically incompatible. Others again may occupy only part of the extension as do the coloured flowers that form the pattern of a carpet. The great condition is that there should be as much continuity within the boundary as possible and as much discontinuity without it. It is to be noted that extension and shape occupy a different place in this notion of a thing from the other qualities like colours, etc. Primarily, extension and shape are qualities of the other qualities. But the bounding shape and the extension contained within it are predicated of the thing as *its* shape and extension. Thus, what is fundamental is the notion of certain sensible qualities having extension and shape. I do not think that this notion can be analysed further. Without some extended quality we have no definite shape or extension, and these qualities can only exist in some shape and extension. Whilst this appears to me to be all that commonsense means by one physical thing at a given moment, it is important to remember that in any particular case a great deal is involved beside direct perception. Direct perception may give us colours in a continuous visible boundary, and it may give us temperatures in a continuous tactual boundary; but we need more than direct perception to tell us that the coloured surface and the hot surface are one and the same thing. For, as we have already noted, it is partly a matter of inference, and partly a matter of definition, to identify a seen with a felt boundary, either as to position or to shape. And then the identification is not a 'finding identical' of the objects of two different sense-perceptions, but a correlation of them with a third unperceived common shape through their correlation with each other.

(*b*) We can now turn to the other objection that at any rate what I have suggested as being what is meant by one thing and the states of that thing at a given moment is not what we do mean, but that it is necessary to introduce the notion of substance and inherence. As we are now talking about the predication of qualities of things it would seem that by 'substance' is to be understood that which can be a subject but not a predicate. Now let us consider a red thing. On our view, to say that it is red merely means that an instance of redness occupies the whole or part of a certain bounded extension within which are a number of other extended qualities and without which for a finite distance there are not perceptible extended qualities of at all the same kind. It also allows us to say that the thing in question is a subject and not a predicate. For, as a matter of fact, the thing never does stand in the same sort of relation to other things in which the qualities that constitute it stand to each other. If it did they would cease to be other things.

But I grant at once that not all propositions that assert qualities of physical things can be interpreted in the way I suggest. To say 'this thing is red and hot' is of the same form as to say 'this thing is triangular.' But if we make the former mean that redness and temperature exist within a certain boundary, we can hardly make the latter mean that triangularity exists within a certain boundary. Yet I do not see that we are forced to say that, since *something* must be triangular and since we cannot interpret the proposition in the same way as 'this thing is red,' therefore we are forced to appeal to substance. For if we ask what is triangular it is not a substance but the qualities. It is the red and the temperature that are triangular. There are, however, some important points to notice about this. In the first place the 'is' here does not mean the same as in 'this is red or hot' when we take the shape as fundamental. Another point to notice is that in the case of extended qualities the 'is' is in a certain sense reciprocal. 'This triangle is red' and 'this red is triangular' are equivalent to each other. And the further development of this brings us to another consideration. It is probably not the universals redness or temperature that are triangular, but 'this red' and 'this temperature.' Of course it is not a completely cogent argument in favour of this view to say that since, e.g., there are red circles and red triangles and since the same thing cannot be both circular and triangular at once, therefore it cannot be redness that has these spatial qualities. Some such complex statement as 'redness is triangular here' and 'redness is circular there,' would meet this objection. If these were analysable into 'redness is triangular' and 'redness is here,' etc., the difficulty would no doubt recur, but there is no reason to assume this. But I do not see that we can possibly avoid introducing particulars somewhere and it is the insistence on particulars which seems to me to be what is true in the substance theory. I would analyse 'this red is triangular' and 'this triangle is red' into this is an instance of red and an instance of triangularity, and it seems to be an *à priori* certainty that any instance of any colour is an instance of some shape, and any instance of any shape must also be an instance of some other universal, such as colour or temperature. What is true in the substance theory then has no special reference to the unity of a thing with a number of qualities but refers to the relations of instances to their universals, a relation which would equally hold in a case of a particular which was an instance of only a single universal.

There is one other point to be mentioned before leaving this subject. Objections have been brought against substance which, if valid, would be equally fatal to particulars. Have they any weight? The objection is that substance is meaningless, that nothing can be said about it. If this be true we could object that particulars are meaningless and nothing could be said about them. The objection is not well expressed. Strictly, a thing has meaning when acquaintance with or knowledge about it either enables one to infer or causes one by association to think of something else. I see no reason why everything should have meaning in this sense. What I suppose to be intended is that the *word* substance has no meaning because no propositions can be known about substance in abstraction, and, therefore, the hearing or seeing of the word does not cause one to think about anything definite. To see whether there is anything in this objection we must consider for a moment what is meant by 'vicious abstractions' which (like most vicious characters) have furnished an inexhaustible theme of conversation especially in Hegelian circles.

The attacks of Berkeley and Hume on abstraction rested on mere mistakes, on the confusion of ideas with images. You cannot have an abstract image, and since ideas were confounded with images, it was thought that you could not have an abstract idea (by which they meant the idea of an abstraction, for of course all ideas as such are concrete particulars). There is then no general objection to abstraction. But there is a common mistake that can be made over abstraction and that is to assume that whenever two things can be conceived separately they can exist separately. And even this must not be rejected without further analysis as a sheer mistake, for it has three forms. The most serious form is to abstract an universal and then expect it to be able to exist like a particular. I should suppose that it was a very rare error. A less culpable form is the following. There seem to be universals so related that an instance of one is always also an instance of the other, and this connexion can be judged *à priori*. For example, to say that there might exist a triangle which had nothing but geometrical qualities would not be to expect triangularity to exist like a particular, for we are talking of particular triangles and making no confusion. Where we err is in forgetting what seems to be an *à priori* law that nothing can be an instance *merely* of triangularity. Finally there are universals that are found in practice always to have gone together, thus giving rise to empirical laws of coexistence. Such are ruminance and clovenfootedness. In these cases it would not be termed a vicious abstraction to assert the possibility of the existence of instances of one that are not also instances of the others. Here 'possible' has the fairly definite meaning of contradicting no *à priori* law.

Now substances and particulars are certainly not vicious abstractions in the sense that anyone supposes that they could exist apart from their qualities and relations. As a matter of fact I think people have been unduly timid in this direction, for I see no obvious objection to the existence of terms that stand in relations but have no qualities except those of standing in the relations in which they do stand. This does *not* mean that they have no nature; they are what they are and they differ immediately from other terms. I have already suggested that we approach (though we certainly do not reach) such terms in tastes and smells. But where people find an objection seems to me in the following place. Where we make an abstraction and it is justifiable, although it may be that the abstracted terms cannot exist apart and are not supposed to be able to do so, yet in order to make the abstraction it is essential that each element abstracted should have a definite nature and that something can be said about it in separation from the other elements. Thus, no one supposes that triangles can exist without colour or something equivalent; yet quantities of propositions can be asserted about triangles owing to the definite nature of triangularity without any reference to the other element which must be present in any existent triangle. But this condition, it will be said, is not fulfilled in the case of substance in abstraction or particulars in abstraction; for, by abstracting their qualities and relations, you have removed all that could be said of them. And it is useless to reply that there is no need to consider them in abstraction because they exist and are given only *with* their qualities and relations. For this very statement assumes that what exists and is given is a complex in which qualities and relations and that which has qualities and stands in relations can be separated in thought. When I say: 'This is red and triangular,' I do not mean that redness is red or triangular or that triangularity is red or triangular and I must therefore be talking about some third thing different from both redness and triangularity.

But does all this really involve any objection to particulars? When we say that we take a particular in abstraction all that we mean is that we leave out of account all the true propositions such as '*X* is red,' or '*X* is triangular,' or '*X* is more virtuous than *Y*,' and confine ourselves to the single proposition '*X* is a particular.' All the other propositions which are true of *X* at all remain so whether we choose to consider them or not. So it is false to say that nothing can be said about *X*; and what must be meant when people say that nothing can be said about *X* in abstraction is that when you confine yourself to saying that '*X* is a particular' you convey no information at all. And this seems to me to be false. Particularity is as good an universal as redness or triangularity; and it is as possible to recognise it. Probably people make a confusion of the following kind. In the first place many universals imply the presence of other universals in any instance of them either in accordance with *à priori* or empirical laws. Thus if *X* be triangular we know that it must also be an instance of some other universal like colour and also that if it be a Euclidian triangle the sum of its angles will equal π. Now the statement that *X* is a particular does not have many interesting implications like this; at best it leads to some not very certain propositions about its relations to space and time, as, e.g., that it cannot be at two places at once.

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But it cannot be an objection to the reality of an universal that it is not connected by *à priori* laws with many other universals, though this fact may make it difficult to recognise and impossible to describe. Secondly, many universals are analysable and can be defined, and people have a tendency to think that there is something disreputable about anything that cannot be analysed and defined; forgetting that if you are to be able to grasp anything at all you must be able to grasp something without further analysis. Now particularity cannot be analysed. The result of these two facts is that it can neither be defined nor described, but must be either directly recognised or missed altogether; though I can hardly suppose that anybody has really missed it, yet the fact that they cannot describe or define it, accompanied by false logical theories which overrate these two processes, has led many to deny that there is such an universal at all.

We can therefore turn to—

(ii) Here we have to discuss the theory that the unity of a thing through time depends on the sensible continuity of the boundary and the qualities within it throughout their changes. It is sometimes said that every change demands something permanent and that this permanent is substance. If the thing at one moment was nothing but sense-qualities in a boundary, how can you say that *it* changes continuously or discontinuously, it will be asked? A good deal of unnecessary difficulty has been found in change owing to the fact that the proposition 'X changes' has been taken too rigidly. 'X changes' seems to present the following difficulty: Whatever else it may mean it implies that X had some [1] quality x_1 at a certain time and that it has now ceased to have it, and either replaced it by another quality x_2 or not at all. Now it is said: Either a thing at any moment is identically the same as its qualities in relations of a certain kind, or we mean by X a permanent substratum. But, if we mean the former, then it is clear that the X of which x_1 was a quality is not the same X which has the quality x_2 and not the quality x_1 so that if, in the proposition 'X has changed,' you refer either to the X that had x_1 or the X that has x_2 your statement is false. The X that was x_1 has not changed; it has absolutely ~~ceased to exist.~~ And the ~~X that is x_2~~ has clearly not changed, for here it is at the moment of predication. (Hence people suppose that they are forced by the fact of change to say that the X that they mean is something different from the qualities in their relations as they are at any moment, that it is, in fact, the common substratum of these successive sets of qualities in relation.)

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[1] I am using quality in a very wide sense here; it will of course include relations

But there is a very short way with this view as a solution of the difficulties. For, if 'X has changed' refers to the substance common to the two successive states, it is clearly false, since *ex hypothesi* the substance is the same in both cases. To this the answer will be that the full statement of the substantial view of change is as follows: 'X has changed' means that there is one substance underlying two successive states, but that these states differ. This, however, does not really help us. For the only way to recognise identity of substance is by identity of qualities; hence, since the qualities at two different moments are, *ex hypothesi*, different in part at any rate, what right have you to assert that the substance is the same?

Accordingly some people thought that they could have change neither with substance nor without it, and so they rejected it altogether. It is clear that the whole question turns on how much sameness is needed to allow us to say that two things perceived at different times and in part different from each other, the same. If people had reflected they would have seen with Hume that nothing like complete identity of qualities is needed to make two things observed at different times to be regarded as the same thing. All that is necessary is that the successive objects of observation should be continuous with each other. By this we mean that, the shorter the interval between the successive observations, the more nearly will the objects perceived at the two moments be found to resemble each other in position, configuration and sensible qualities. When no difference can be found in two successive observations we say that what is observed has not changed in the interval. When we have perceived successive differences and yet continuity in the sense which I have mentioned we say: Such and such a thing has changed. By this we do not mean that any sort of substance has remained permanent all through, but we mean that we have either observed or have reason to believe that we could have observed that the whole process was continuous in the sense mentioned above from that stage which we now observe to that one which we observed some time ago and called 'This *X*.' Thus our theory is perfectly competent to account for the statement that a thing changes and yet is permanent; whilst the substance theory adds nothing useful to our own.

We can now return to the subject of the relation of causal laws to things, since we now know what is meant by things and their states. We find from our discussion of the meaning of one thing a considerable support for our rejection of the view that causation means that one thing forces another to act in a way in which its own nature would not let it act if it were left to itself. For we now see that what constitutes one thing in the physical world is nothing of a deep or recondite character, and that its nature is just its qualities from moment to moment in the relations in which they constitute a thing. These successive qualities in relation still remain precisely what is meant by the nature of the thing even though they would be different if it had different relations to other things. We saw that, so far from causation involving two separate natures and the forcing of one by another, it was ultimately only *after* the introduction of causality and by means of it that we could theoretically define what a thing does of its own nature, and that even then this definition is doomed to remain purely theoretical. In spite of this, since the notion of thing is of great importance, it is in general true to say that it is necessary to state causal laws not merely as holding between successive qualities, but rather between the successive qualities of definite sorts of things, that phrase being defined in the sense that we have given. For in general it will not be irrelevant to the truth of a causal law whether the various states mentioned in it occur in the same or in different complexes.

We can now express what is meant by the distinction between immanent and transeunt causality and can see that there is no difference in principle between the two that would justify the immense distinction that Leibniz and Lotze make. Immanence is in the first instance always relative to a definite system, and a definite set of events. A causal law is immanent to a given system S when all the data that are required by it are to be found within S at various times and all the states that can be inferred are states of parts of *S*. But *S* may be a system with many different parts. Then relatively to the same causal law that law will be transeunt with respect to any part of *S* that we like to choose, because to infer its states we shall have to consider the states of other parts of *S*.

Thus an isolated system of particles in Mechanics is a system all of whose configurations are determined by causal laws immanent to the system, but the position of any particular particle in it is determined by laws which are transeunt with respect to it. This is the true distinction between transeunce and immanence. But the notion of one thing enters in the following way. The isolated system under immanent causality may also under our definition be one thing and then we come to the more common-sense of immanence as Lotze and Leibniz use the term. On the other hand the system in which immanent laws of causation hold may very well be a selection of separate things in our sense, as indeed is the case with the isolated mechanical system of particles. In such a case Leibniz and Lotze would have overlooked the immanence in the whole system, just because the system was not one thing, and fastened on the transeunce within it with respect to its various elements. But our discussion as to what is the criterion of one thing will enable us to see that both sides are equally valid and that there is no possible ground in the nature of thing to make causality immanent with respect to it intelligible and causality transeunt with respect to it a mystery or a contradiction. Our main conclusion then in this section is that anyone who can accept immanent causality has no right to strain at transeunt causality; and when it is remembered that it is the straining at transeunt causality that is one ground of Leibniz's monadism and the sole ground of Lotze's belief in *M*, this result will be seen to be of some importance.

I now pass to a second question about Causation, viz.:—

Causal Laws and Time. On this relation of causation to time many of the arguments against causal laws have been based. It was asserted by Hume that cause must be prior to effect and contiguous to it in time. The latter phrase however implies that there are contiguous moments in time and this contradicts the general belief that the time series is continuous. Thus if there were reason to suppose that in any causal law, cause and effect must be contiguous there would be some grounds for denying that there can be any causal laws. Hume, however, offers no argument for his position. He just says quite bleakly, that 'nothing can operate in a time or place which is ever so little removed from those of its existence[1].' For a person who was so convinced of the value of Newton's law of gravitation as to wish to introduce something like it into the mental world the statement with regard to space seems an odd one, and, as that with regard to time has no evidence for it and is incompatible with that continuity of time in which most people believe, we need not consider that we have here an antinomy. We ~~need not reject~~ causation, but only Hume's doctrine that cause and effect must be contiguous.

This leaves us with the alternatives that they are continuous with each other, that they are contemporary, or that they are separated by finite intervals. At this point Bradley appears on the scene with an argument against causation which attempts to prove that cause and effect both must and cannot be continuous with each other. The arguments are (i) Cause and effect must be continuous with each other, for otherwise there will be a blank time between them during which the cause does not operate. And, if it does not operate for a finite time however short, why should it ever do so? (ii) Cause cannot be continuous with effect, for, if so, then if we take an infinitely thin section across the stream of events it will contain the cause of all that follows; and yet, since it will occupy no time at all, it will not be an existent[1].

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[1] *Treatise on Human Understanding*, Part III, § 2.

(i) The thesis must either be supposed to assume the activity view of causation, which we have rejected, or it will need to be greatly modified. As it stands it assumes that the cause forces the event to happen as soon as it is strong enough to do so. If then it was not strong enough to do so at the beginning of the interval and nothing happened to it in the interim it is impossible on the activity theory to understand how it came to be more successful at the end of the interval. If, on the other hand, it became strong enough at the end of the interval then the last cause is the state at the end and not at the beginning and so the cause once more is continuous with the effect. After our rejection of the activity view of causation such arguments will leave us very cold. It is of interest to note that Sigwart has an almost identical argument which is so honestly and clearly put that its futility becomes obvious to the most casual reader. He says; 'If effect consists in a change, and if we can speak of causation only in so far as a change occurs, if therefore nothing is effected until the change occurs, then the action of the cause and the resulting effect must necessarily be simultaneous[2].' This is word for word what we have made out of Bradley's argument for the thesis. It comes simply to this, that a thing is not a cause unless it produces an effect and therefore you have no right to call it a cause until it has produced its effect. It is clear that such an argument is merely verbal, and it is not worth while to consider it any further.

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[1] *Appearance and Reality*, pp. 60—61.

[2] *Logic*, Vol. II. p. 104.

Since the thesis, when stated on the activity view, becomes a mere play upon words we will see if any better fate awaits it when stated in terms of a descriptionist theory of causation such as we are trying to defend. Stated in terms of the latter view, Bradley's argument would be: No state that persists for a finite time can be a datum in a causal law from which the state which begins when that time is over can be inferred. The first thing to be said of this proposition is that if it happened to be true, it would be a very miraculous thing that people should ever have thought that they had discovered and verified causal laws. For it is tolerably certain that no one has ever been acquainted with a state that lasted for no time at all. At best he can only have taken a kind of average over a short duration. The second point is that the proposition gets such plausibility as it has by being confounded with the other proposition that nothing that has been completely quiescent for a finite time can have its subsequent changes accounted for by purely *immanent* causation. But that does not at all mean that a persistent state in a system may not be one of the data in a law of transeunt causation connecting this state and those of other systems with future states of the at present quiescent system. If a billiard ball ~~be~~ is at rest there is no purely immanent law of causation connecting its quiescent states with any future movements that it may have. But its mass and position, though they have persisted for a finite time unchanged are all-important elements in a transeunt causal law connecting its future positions with those of another billiard ball that I have just hit in its direction.

I think the above discussion will enable us to see exactly what is to be accepted in the doctrine that cause and effect are continuous, and subject to what assumptions it is to be accepted. (i) It is *assumed* by common-sense that no system that has been completely quiescent for a finite time can have its future changes, if any, predicted by purely immanent causal laws. This I think must be taken to be a belief about causation which most people would consider self-evident. (ii) It is quite admitted that the change in B, which is one of the data from which the states of A, subsequent to its quiescence, can be inferred by a transeunt causal law, need not happen at the moment at which A starts once more to change. (Indeed, if time be continuous there is not a moment at which A starts to change. A is α for all moments after t and before t' and β at t' and moments after it; so that there is either no last moment at which it is α or no first moment at which it is β.) But if there be a finite gap between the occurrence of the event in B and of the new one that can be inferred in A, then taking the systems A and B together they will be a system within which this particular causal law is immanent. Hence the whole system $A + B$ will have been quiescent for a finite time and we shall have to seek a third system C for a law transeunt with respect to $A + B$ which shall account for its emergence from quiescence. To put it in symbols the maxim (i) holds that if a system A in which there has been no change for a finite time τ takes on the new quality

β at the end of that time then we must go outside of A to another system B and a state b in it to account for the occurrence of β. But if b occurs some time within the interval τ and B remains quiescent for a finite term in the state b then, if we take A and B together as one system, that whole system will be quiescent with the states b and a for a finite time. If the original maxim be true this demands an appeal to a third system C to account for the emergence of $A+B$ from this quiescent state. Hence the maxim (i) compels you to press forward until you have reached a system which includes A in which something is happening through the whole of the interval τ which in A alone is blank and part of which in $A+B$ is blank and so on. Thus if we accept the maxim that the future changes of any quiescent system that follow its quiescence can only be explained by transeunt causal laws, we have to say that the complete account of any case of causation compels us to proceed until we reach a continuous causal series immanent in a larger system which includes A. I must confess that the maxim does seem to me self-evident. I cannot believe in an immanent causal law of the form: When the system A has been in the state a for $2\frac{1}{2}$ minutes we can infer that it will become β independently of anything that happens in the rest of the universe. I the~~refore have to accept~~ the conclusion of Bradley's thesis, though not his argument for it, and to suppose that in the end all causation is continuous. Let us pass then to

(ii) This attempts to prove that causation cannot be continuous. This is much wider in scope than the thesis; and here again Bradley's argument seems to rest entirely on the activity view of causation for its plausibility. Each infinitely thin section is to cause the next and yet, being infinitely thin it will occupy no time and so not exist. In the first place we must not put the argument as I have just done in the form that each section causes the next, for that would be to revert to the theory of contiguity which we have rejected. We are assuming continuity of time and so there will be no next. But I suppose that the real point of the argument is that all your data will be states without duration and that this is a vicious abstraction. We must distinguish two different sorts of objection to states without duration: (*a*) Metaphysical objections, and (*b*) Epistemological ones.

I have already hinted at the epistemological objection in discussing the thesis. How could you discover causal laws, if their data are states without duration, since you certainly cannot directly experience such states? But here we have the metaphysical objection: How could such abstractions be the causes of anything? (*a*) It seems to me that the metaphysical difficulty which is the only one on which Bradley touches is not really a serious one. It is only when you take the view that the cause must be an active substance with all the solidity of chairs and tables that there is any difficulty in the fact that events without duration claim to be causes. When you merely mean that if you know enough events at enough moments you can infer some events at all moments the relation all through is a semi-logical one between these thin events which are at a moment but do not endure for a time, and there is nothing very startling about it provided you can persuade yourself that there can be states without duration and that you can know them. But certain metaphysical questions about change are raised. This question is evidently a general one connected with change and not with causality in particular. We may best approach it from the point at which we left change when we discussed what was meant by one thing and by its altering and yet remaining the same thing. ~~For~~ our explanation of the changes of things which are complexes of qualities in relation assumed the changes of qualities and relations, and questions may arise over the latter as well as the former. A piece of iron, let us suppose, was black; it is heated and it becomes red. This we explained to mean that a certain complex of qualities altered in a certain well-defined sense. But we did not discuss what was meant by the black changing to red and all the time remaining a colour. Change of qualities and relations may be of two kinds. Either a quality is a member of a complex for a time and then ceases altogether to exist in it, or else we have continuous change of a quality. As examples of the two sorts of change of quality we may take (*a*) an overcooled liquid suddenly crystallising, and (*β*) a body steadily rising in temperature. (*a*) With the overcooled liquid the quality of fluidity has been present for a finite time. A crystal is introduced and suddenly it solidifies. The quality of fluidity has thus ceased to be a quality combined with the other members of the complex and has been replaced by that of solidity. To avoid complications we will confine ourselves to the change of tactual qualities that has thus suddenly taken place. The two successive tactual qualities are synthetically incompatible; they cannot be attached at the same time to the whole of the same bounded extension. The extended fluid quality has ceased to occupy the given boundary and it has not appeared anywhere else. This kind of change then involves that a quality may exist for a finite time in a given boundary and then cease to exist there altogether. And another quality that did not exist there before may begin to do so at a moment such that at all moments before it and after an earlier one the other quality was there. I do not think that any analysis can be made of what is meant by existence with a certain boundary during a certain time. The only question then is whether there is any difficulty in saying that a certain quality exists for a certain time at a certain ~~place and that at~~ other times it does not exist there. It will be said: if you really mean the *same* quality how can it have different relations at different times? How can it both exist here and not exist here? This difficulty is I think met by Mr Russell's[1] position that there can be no objection to a quality having a relation to certain moments of time and points of space which it does not have to other moments of time and points of space, just as there is no objection to the Senate House being to the left of King's and to the right of Trinity when viewed from the river. It is proper to note that it is not necessary to drag in absolute time with moments in order to recognise this as the correct solution of the problem. The temporal relations of events have magnitude. Hence we can say that there is no objection to fluidity having that complex relation to existence and the Norman Conquest and a certain boundary which is called 'existing here from a time τ after the Conquest to a time τ' after it' and not having the relation to the same terms called 'existing here from a time τ' after the Conquest to a time τ'' after it.' For there can be no reason why the same set of terms should not have some relations of a kind and not others of that kind. Thus the principle of Mr Russell's solution remains the same whether we have an absolute time series as he supposes or only relative time. On the former assumption change means that a quality has *the same relation* to existence and *various* moments of time and does not have it to other moments of time. On the latter the *same entities* have some but not all of the relations of a certain kind to each other.

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[1] *Principles of Mathematics*, Chap. LIV.

(β) We can now, therefore, leave the question of sudden changes from a quality that has persisted for a finite time, and come to continuous change which leads to Bradley's difficulty. We are going to take as our example a body steadily rising in temperature. This involves that at no moment is the degree of temperature the same as at any other moment, however near. Now only definite degrees of temperature exist. But if the change be continuous no definite degree persists for any time at all. If it exists it only does so at a moment and not through a duration. And so far as I can see the plausibility that is left in Bradley's antithesis after we have rejected the activity view of causation rests on the belief that if none of the successive degrees persist none can be said to exist. But if none of the particular degrees existed then clearly there was no temperature in existence throughout the whole of the interval during which it was said to be changing continuously. The argument then rests on the assumption that anything that exists in time must persist for a finite time. Can this axiom be maintained?

The result of holding it would be that we should have to believe that all changes are of the kind discussed in (α), i.e. that each state lasts for a finite duration and is then succeeded by another. When the duration of each is less than a certain amount, the change has for perception that peculiar character which we call perceptual continuity. If space be continuous this will mean that a moving body cannot pass through all the points on a straight line. For motion would involve that a body was at A for all moments not before t and before $t + \tau$ say, at B for all moments not before $t + \tau$ and before $t + 2\tau$, etc. If then it were at all the points of a straight line and the line were continuous it would take an infinite time to cover any distance however short. But it is perfectly possible to assume either that physical straight lines are not continuous but consist of large numbers of contiguous points very close together, Or we can assume that the lines are continuous but that a moving body only visits a selection of their points. Similarly we can deal with temperature or any other physical magnitude that seems continuous. They may be only a large but finite number of different degrees of temperature, or else any given body rising in temperature may only touch a finite selection of the infinite number of possible intermediate temperatures each of which perhaps actually exists in some body at some time.

Would this give as much continuity as the acceptance of the axiom about immanent causality, which we discussed in the thesis, seems to necessitate? That axiom forced us to hold that the total state of the universe could not be the same for any finite time. Now the present axiom would hold that every state of everything in the universe persists unchanged for a finite time. For these to be compatible it would be necessary that every moment should be the last of some state in the universe though all states endure for a finite time. Now there is no objection to this being the case if there be enough states in the universe. If the time series be compact and there be $2^{\aleph_0}$ states then each state can endure for a finite time and yet the universe never is in the same total state for a finite time however short. Hence the supposition that the states of every particular thing in the universe change discontinuously is not incompatible with the result deduced from our earlier axiom about causation if only there be enough states in the universe.

It would follow, however, from our original axiom that no immanent causal laws for the causation of continuous changes of position or degree of quality would be possible. Thus, if motion be really discontinuous, so that a body moving with a fixed velocity really stays for a short but finite time at the consecutive points of a discontinuous space and then without any lapse of time is at the next point and so on, the first law of motion which is apparently immanent must really be pseudo-immanent and the result of a more fundamental law of transeunt causation. For taken as an immanent law it would be of the form: After the material point X has been at a point of space P_n for a short but finite time τ it will be at the consecutive point P_{n+1}. This form of law our axiom rejected as pseudo-immanent. Hence the assumption that motion is discontinuous is incompatible with the belief that the first law of motion is an immanent law and as such applies to an isolated system. There are not of course the same objections to the assumption that the change of physical qualities, like temperature, are really discontinuous, because their changes are not supposed to be subject to immanent causal laws.

There is, so far as I am aware, only one way of dealing with a very fundamental proposition such as: 'Whatever exists in time must persist for a finite time,' which is neither obviously true nor obviously false; and that is the rather tedious one which we have pursued of seeing what would follow if it were true and then trying to judge whether these results make that which implies them probable or improbable. Our investigation has shown (1) that the argument has no particular relevance to causality when we drop the activity view of causation, but is an argument that really deals with the continuity of change. (2) But an axiom which seemed self-evident about causation did imply that at any rate the states of the universe as a whole must change continuously. We saw, however, that if the universe only has enough states this is compatible with the view that the states of all that is in it last for a finite time. Hence there is no antinomy whether we assume continuity or discontinuity in the changes of particular things. (3) As the proposition does not appear to me to be in the least self-evident, and as it is incompatible with the view that the first law of motion is the truly immanent law of an isolated system I see no reason to accept it. But we have seen that there is no antinomy over causal laws whether we accept or reject it.

We can therefore turn to—

(*b*) This it will be remembered is the alleged epistemological difficulty raised on p. 111 against the statement of causal laws in terms of momentary states. The difficulty was that the laws of causation are supposed to be about the existent and to be found by observing the existent and yet there is not only the doubt whether momentary states exist, but the certainty that they cannot as such be observed. But surely, even if we accept the view that the laws of causation are stated in terms of momentary states and that these cannot be observed and do not exist, we have no reason for concluding that causal laws may not be true of the existent and be deduced from observations upon it. Granted that no one perceives momentary states it must also be remembered that no one *perceives* causal laws, any more than he perceives states that do not endure for a finite time. Both causal laws and the momentary states into which perceptually continuous change of quality is analysed are discovered by reflecting upon and reasoning about what we do perceive. It then becomes quite irrelevant to the validity of causal laws whether the ~~states in~~ terms of which they ~~are formulated~~ really exist, so long as it is always possible to retrace ~~one's~~ steps from these durationless states to what we actually do perceive ~~which~~, ~~for~~ that reason, ~~exists~~ and endures. Now this, I take it, we always can do. We know how we have reached the momentary states from the enduring ones that we perceive and hence we know how to get back from the former to the latter. The argument would run somewhat as follows. We can get a conceptual account of what is meant by continuity or compactness by considering the series of real numbers or the series of rationals respectively. And here we are on safe ground because we are acquainted with the elements and can define them. Now there is no ground for thinking that time is *more* continuous than the series of real numbers, in fact there is no reason for attributing to it anything more than compactness which the series of rationals possesses. We cannot become directly acquainted with moments of time, but if we suppose that finite durations consist of terms without duration and that the relation of before and after among them has the same logical qualities as that which generates the compact series of the rational numbers we know that we cannot have erred on the side of attributing too little continuity to time. Hence we can define moments as terms having the same kind of relation to finite times as rational numbers have to certain classes of them. Suppose then that we do attribute this kind of continuity to the time-series. What then are we to say of states that remain unchanged for a finite time? Clearly they can be analysed into the same state at each of the infinite number of moments within the finite stretch. Hence we get to the notion of momentary states. Now the important point to notice is that no logical exception can be taken to any of these steps if rightly understood. Time is held to be continuous in the same general kind of way as the rationals, hence it cannot be wrong to get clear ideas about the continuity of time by analogies with the rationals where we actually know what the elements are. The assumption that time consists of moments arranged by the same kind of relation as the rational numbers will certainly account for all the continuity that is perceived. This being granted, no exception can be taken to the analysis of existence through a finite time into that of existence at each moment within the stretch. It is never necessary in the case of causal laws to think of this analysis as being any more than a form of statement which can always be translated back into the only form in which we do perceive states, viz., as lasting for a finite time.

Clearly most if not all causal laws are discovered from our perceptions. In the case of states that last for a finite time the momentary state is precisely the same as that which persists and can be observed, and so no difficulty ought to be felt. In the case of continuously varying states what we mean by the momentary state is that which would have persisted if the process could have been stopped at the moment at which the state is said to exist. In practice we try to take permanent records and thus shift the impossible problem of directly determining a momentary state into the soluble one of determining a momentary state when precisely similar ones exist at all the moments of a finite duration.

The upshot of the whole discussion is that, although we admit that causal laws apply to the existent, and though they are certainly stated in terms of momentary states which cannot be directly observed, it is quite unimportant for our theory of causation whether momentary states really exist or not. Momentary states are the product of a theory of time which (*a*) will account for the facts, and (*b*) is internally consistent. The theory connects its momentary states with the enduring ones that can be observed in a perfectly definite and unambiguous way. Hence it is perfectly possible to interpret laws connecting momentary states in terms of states that can be observed and *vice versâ*. And so, whether momentary states exist or not, it is perfectly possible (*a*) from observations on the changes that can be perceived to pass to laws stated in terms of momentary events; and (*b*) to pass back from these laws to foretell what states will actually exist in the world.

We can pass then to another question as to the relation of causal laws to time, viz., *Is Causation always successive?* It has been held by many philosophers that the cause must always precede the effect in time. Hume attempts a proof of this, and Lotze has a similar argument. Hume's argument runs as follows: "'Tis an established maxim both in natural and moral philosophy that an object that exists for any time in its full perfection without producing another is not its sole cause; but is assisted by some other principle which pushes it from its state of inactivity....Now, if any cause may be perfectly co-temporary with its effect, 'tis certain according to this maxim that they must all of them be so; since any one that retards its operation but for a single moment exerts not itself at that very individual time at which it might have operated, and therefore is no proper cause. The consequence of this would be...the destruction of that succession of causes which we observe in the world, and indeed the utter annihilation of time. For, if one cause were co-temporary with its effect, and this effect with *its* effect and so on, 'tis plain that there would be no such thing as succession[1].'

We have seen in what sense the 'established maxim' is to be accepted. We must now see precisely how Hume's argument depends upon it, and whether it would be valid with the form of the maxim that we have accepted. The essential point is the attempted proof that, if any cause be contemporary with its effect, all must be so. ~~This is supposed to follow~~ from applying the maxim to a given cause supposed to be contemporary with its effect. We must remember that Hume believes that there are contiguous moments in time. Hence the alternative for him is between a cause producing its effect at the next moment, or at one separated from it by several moments, or at the same moment. The maxim as stated by him cuts out the middle alternative. He evidently does not think that it cuts out the first alternative, because that is as a matter of fact his own view. He wishes to prove that, if any cause produces its effect at the same moment as itself the maxim will show that all causes must do so. Now he only does this by assuming that there is no alternative between a cause producing its effect at the same moment as itself or at several moments later. This of course is true if time be continuous, but is inconsistent with his own position, since he accepts the view that the cause 'does not retard its operation for a single moment,' which means that it produces its effect at the next moment. But whether we accept next moments in time or not it is obvious that his argument is formally vicious. It is perfectly obvious that the two premises 'Some causes produce their effects at the same moment as themselves' and 'No cause produces its effect at a moment later than the next' cannot possibly lead to any conclusion about all causes. Yet Hume makes them lead to the conclusion that all causes produce their effects at the same moment as themselves. Hence his argument is invalid whether we accept his premises or not.

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[1] *Treatise of Human Nature*, Part III. § 2.

It is interesting to compare Lotze's argument with Hume's. Lotze[1] is trying to produce an antinomy, about Becoming, and he does this by way of simultaneous causation. He argues as follows. If causes be simultaneous with their effects their relation reduces to logical connexion. This is similar to Hume's position. But as the antithesis of his antinomy he takes the argument: If causes be not simultaneous with their effects then a cause would be able to exist for a finite time without producing its effect. On contraposing this antithesis we see that, for the same maxim, Lotze deduces that all causes must be simultaneous with their effects, whilst Hume concludes that, if any cause be simultaneous with its effect all must be so. We can now see the reason for the difference. Hume's argument is sheerly wrong and Lotze is right on his own premises. Lotze's premises are that two events must either be separated by a finite time or be contemporary and Hume's maxim. These two do lead to the conclusion that all causes must be simultaneous with their effects.

But it is easy for us, after our previous discussions, to see that any argument for or against simultaneous causation based on the maxim cannot be accepted as it stands. It is perfectly clear that both Hume and Lotze assume in their arguments the activity view of causation that we have rejected. The former seems hardly justified in doing this in view of his final conclusions. But the form in which we have accepted the maxim leads to no conclusion whatever about successive or simultaneous causal laws. The phrases 'the cause' and '*its* effect' lose most of their meaning when we turn all causality into causal laws by which inferences can be made from a certain number of data to others at different times. The maxim as accepted by us merely assured us that a causal law of the form: 'X remains in the state a for the time τ unchanged and then passes into the state β' is not an ultimate law immanent to the system X but will always be found to depend on laws transeunt to X. But this does not tell us that the data in causal laws, whether immanent or transeunt, must be contemporary with that which is to be inferred. In fact it is quite clear that as a rule they are not. The only question then for us is whether causal laws ever connect states that happen at the same moment.

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[1] *Metaphysic*, Vol. II. § 207.

Lotze and Hume both seem to think that this is impossible; but they both hold, though on different grounds, that *all* causes would be contemporary with their effects and discuss the question on that basis. Since we have no need to accept this view, and indeed no possibility of doing so we cannot accept the *reductio ad absurdum* of these philosophers that there would then be no process in the world. On the other hand the thesis of Lotze's antinomy would apply to all cases ? of simultaneous causation if it were valid, even though not all cases of causation were simultaneous. We must therefore consider it. Lotze's argument is that simultaneous causation would be nothing but logical connexion. When we say that there is simultaneous causality we recognise that there is also a process in time, and if the causality be really simultaneous there could not, Lotze thinks, be a process in time.] If this argument be true it will be fatal to any case of simultaneous causality that offers itself. It is most certainly true that when we commonly hold that there is simultaneous causation we also hold that there is a process, just as much as where we believe the causation to be successive. Thus it would commonly be said that the heat of a fire is the cause of water's boiling, and that once the water has started to boil it is the heat of the fire that causes by simultaneous causality the continued boiling. And the fire's burning and the water's boiling are both continuous processes that occupy a finite time. Now is it true that if the causation were really contemporaneous with the corresponding evaporation of the water there would be no process?/ On our view of causation to say that there is simultaneous causality would be to say that there is a law connecting each momentary state of the fire with the state of the water at the same moment. If then the change in the fire and in the water that goes on through a finite time be nothing but the succession of their momentary states there can be no objection to the statement that there is at once simultaneous causality and a continuous process in the fire and the water. Hence the burden of the argument rests on the old question: 'Can the process in the fire and the water be resolved into the fact that they have different states at each moment of time, states that do not themselves occupy any duration?' This question we have already discussed in reference to Bradley's argument about the continuity of causation, and we saw no reason to answer it in the negative when once we understood its meaning.

But Lotze has an argument of his own on the subject which we must just notice. Lotze attempts the same solution as we have offered and then says that it will not do. His argument is as follows: 'If we assume, as was assumed, that *C* (i.e. the cause in Lotze's notation and the fire with its successive states in our example) traverses the series c_1, c_2... then, as the order of the series is supposed to be fixed, each term must be the condition of the succeeding one, and, as in the previous case (i.e. the problem of which this account is offered as a solution) two adjacent terms can neither have any blank interval of time between them nor can they be simultaneous[1].' Thus the argument against the attempted solution is that the same difficulty breaks out inside the successive series of states which are supposed to be causes in the simultaneous causation. The argument assumes that in any continuous change each state causes the next one. But, as we saw, if the change be continuous there can be no next states. Further we saw that there is no need to suppose that if the continuous change were subject to immanent causation that causation must be simultaneous. No doubt, *if* it were simultaneous there would be no process; or to put it in another way, the successive states of a system could not be accounted for by immanent simultaneous causality. But, since we saw no reason to suppose that the amount of continuity that causation demands precludes causal laws connecting states that are separated by finite intervals of time, we are not faced by Lotze's dilemma. I conclude then that, if our earlier discussions about the continuity of causation be accepted, there is no need for us to deny that there may be laws of simultaneous causality and that they may be perfectly compatible with a continuous change of state in what is called the 'cause' in common language.

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[1] *Loc. cit.*

It being granted that there is no objection to simultaneous causality two questions remain about it: (i) Suppose that there is such causality how do we distinguish cause and effect in it? and (ii) Is there much reason to suppose that there is such causality?

(i) With regard to the first question it is obvious that people do claim to distinguish cause and effect even when they believe themselves to be dealing with cases of simultaneous causality. Thus no one would say that the evaporation of the water caused the heat of the fire. With the theory that causation reduces to causal laws we must remember that the use of the term 'a cause' differs very much from that which is implied by the activity view and is therefore the usage of everyday life. Here we just have laws that unite the existence of states in certain aggregates and relations with that of other states at the same or different times in the same or different aggregates. Which of the states then are to be called the causes and which the effects?

The distinction I think depends on the fact that many causal laws are not convertible. We may know that if x and y occur at t_1 and t_2 in A and B respectively then z will occur in C at t_3 whatever moments t_1, t_2, t_3 may be, so long as $t_2 - t_1$ and $t_3 - t_2$ are fixed. On the other hand the occurrence of z in C at t_3 will not in general enable us to infer x and y in A and B at t_1 and t_2 respectively. When there is this lack of reciprocity we can call x and y in A and B the cause of z in C. But all this is really very unimportant and there are plenty of cases where it cannot be employed, as will be seen when we come to discuss the actual nature of causal laws. So far as it holds at all it

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applies equally to simultaneous causality, which only differs from the more general example given by its relation to time. Thus we do say that the fire boils the water because given the fire in the proper relation to the water we can infer the boiling of the kettle, but given boiling water we can only argue to a fire as one among a number of other possibilities. On the other hand when the causal law becomes strictly reciprocal I doubt if it be possible any longer to give a reasonable meaning to the distinction between cause and effect.

Perhaps one that agrees more closely with common usage for the case of simultaneous causality than the one that I have offered would be the following. Suppose that a system, like the water, has been quiescent for some finite time and it then begins to change. Then we know that the law of causation for these changes must be transeunt to the quiescent system. If the causality be simultaneous, but leads us to states in another system, which was changing with respect to these states while the first system was still quiescent, we shall generally call the changes in the latter system the cause of those in the former. Thus the water was at a certain temperature for some time. It changed and the law of this change leads to another system, viz. the fire, states in which produce the changes in the water by simultaneous causality. But, the process in the fire was going on while the water was still in its quiescent state. Hence we say that the fire causes the changes in the water and not *vice versâ*.

(ii) The second question is whether there is any strong reason for believing that simultaneous causality takes place. It is clear that, if there be any causality at all, some of it must be non-simultaneous, and therefore it would be an aesthetic advantage if the apparent

xvi.
Bradley
surely uses
common + other
necessary qualifications!

cases of simultaneous causality could be reduced to those of successive causality. I do not think that it would be possible to offer any proof of the existence of simultaneous causality, because all the supposed cases might really be examples of non-simultaneous causality in which the interval between cause and effect was so short as not to be perceptible. We do not, I think, use the term causality unless there be a change going forward. Thus when we say that all ruminants are cloven-hoofed we do not generally mean to assert that one characteristic causes the other by simultaneous causality. Nevertheless, if we accept simultaneous causality at all there is no difference in principle between the connexion of cloven-footedness with chewing the cud and that between the momentary states of the fire and the water. The only difference is that in the former case the cloven-footedness is the same at all moments within a finite time, whilst, in the latter, the states of the fire differ at every different moment. In both cases the actual connexion between momentary states is that one state at one moment can be inferred from the other state at the same moment. The resemblance between what is believed to be simultaneous causality and one attribute being a mark of the presence of another is so great that it will hardly be possible to insist that the former cannot really be simultaneous without holding that the latter cannot be so either. I do not think that anyone supposes that constant connexion between attributes in the same thing apparently at the same moment is really a case of a connexion between momentary states separated by a very short interval of time. But this inferrability of one attribute from another at the same moment in the same thing is precisely what would be meant by simultaneous causality immanent with respect to that thing. Hence unless they make this assumption in the case of constant coexistence of attributes in one thing there seems to be no ground for making a similar assumption when we are faced with what is *primâ facie* transeunt simultaneous causation. It is never absolutely certain that what appears to be simultaneous may not really be separated by a short interval of time, but, as there is no theoretical objection to simultaneous causality, and as what amounts to immanent simultaneous causality is commonly supposed to be really simultaneous, there seems to be no good ground for rejecting simultaneous causality in general.

We can pass then to another quite general question about causal laws: *What is meant by the Necessity of Causal Laws?* Causal laws have been reduced by us to regularities connecting ~~together~~ the simultaneous or successive states of things. (1) But it seems clear that there are some regularities of this kind that are not held to be causal. (2) Again we constantly find that causal laws as stated are not obeyed. Something, we say, has 'interfered.' In what sense then can those laws that are admitted to be causal be held to be universal and necessary or indeed even true? We will discuss these questions in order, and we shall find that they have a good deal of connexion with each other.

(1) With regard to simultaneous causality we should hesitate to say that chewing the cud was the cause of cloven-footedness in animals or *vice versâ*; and, with regard to successive causality we should not say that the night causes the day or conversely. Yet we saw that the connexion between chewing the cud and cloven-footedness was, in our sense of causality, apparently a case of simultaneous immanent causation.

There is much less certainty about the sequence of night and day even seeming to be a case of causation in our theory. It may be doubted whether the occurrence of darkness is a datum from which that of light can be inferred without a good many further data.

We must expect that, as our account of causation drops activity and so differs from that of common-sense, we shall sometimes differ from common-sense in our application of the term. In the example of cloven-footedness and ruminance I think that we must admit that we have a very specialised case of a causal law. But it remains true that a great many regularities are not held to be directly causal, and it will be valuable for us to investigate this point.

The important thing to notice is that we deny the term causality to regularities which are known or believed to be analysable into other regularities. The fact that day follows night is not thought to be an instance of a causal law because it is analysed into (*a*) the fact that the earth turns round on its axis once in twenty-four hours, (*b*) the fact that the light on the earth is due to the sun, and (*c*) the fact that the earth is opaque to light. This is not of course an analysis in the sense that all these characteristics can be found in the mere fact that darkness and light regularly follow each other. On the contrary, the various propositions are got from various facts which they explain or from direct experiment, and together they account for the observed regularity. The facts to be explained are the successive appearances and disappearances of the sun which is observed to travel across the sky from east to west and ultimately to disappear below the horizon. We know by analogy with other opaque bodies and other sources of light that this can be explained by supposing that the earth turns round relatively to the sun. The point to notice is that our original regularity has been analysed into a regularity in the relative motions of the earth and the sun, and another as to the invisibility of sources of light through bodies like the earth. Further considerations lead us to believe that the successive positions of the earth depend on its past positions. The analysis cannot, so far as we know, be carried further and so these regularities are finally taken as causal.

Thus we seem to find that a regularity is not held to be causal unless it cannot be analysed into other regularities, but it is always held that the products of the analysis will be one or more laws of causation. On the face of it this distinction between causal and other regularities seems to raise more questions than it answers. It may be reasonably asked whether (*a*) it does not become purely subjective in view of the fact that we never know whether a given regularity may not be further analysable; and (*b*) some further account of analysis is clearly demanded.

(*a*) The first objection is not of importance. No doubt all our knowledge is subjective in the sense that we do not know how much we have yet to learn. But here this is unimportant because, whilst we may learn that something which we took to be a causal regularity is really analysable and therefore not truly one, we shall not learn that what we judged not to be a causal regularity really is one. If we can give some account of analysis which will stand criticism, then we can say that every further step in our analysis of regularities brings us nearer to truly causal laws, even if we can never be quite certain that we have got them. It is right to remark in passing that a good many physicists (including Helmholtz), and some philosophers (including Sigwart) seem to have held that there were marks by which causal laws, in the physical world at any rate, could be recognised. Such laws, they thought, would be ones connecting homogeneous terms with permanent qualities which only differed by their relative positions in space. Even if this were true of the physical world we can hardly call it a complete account of the nature of causal laws in general. For it assumes that the real world of matter is much less varied than what we perceive, and for the qualities dropped out we shall need laws connecting that world with our minds. And it is clear that the latter cannot be reduced to regularities among homogeneous things with permanent qualities which only differ by their relative spatial positions.

(*b*) The question of what precisely is to be understood by analysis of regularities in the present connexion is a more serious one. There are several ways in which a regularity may be analysed.

(*a*) By splitting it up into two or more regularities which occupy successive stretches of time into which the period of the original regularity may be split. Thus, take the regularity that, if gunpowder be hit hard enough, it will explode. There is strong reason to believe that the period between the blow and the explosion is the scene of two successive regularities: (i) the blow is followed by rise of temperature; and (ii) the rise of temperature, if it passes a definite limit —a fact which itself depends in part on the violence of the blow—is followed by an explosion. These subsidiary regularities approach much more nearly to the causal ideal than the regularity which is analysed into them. For the blow may not be hard enough to give the requisite rise in temperature or the heat may be conducted away too quickly, and then the complex regularity will fail although the simpler ones will still hold.

(β) The second way in which a complex regularity may be analysed is into contemporaneously operating causal laws, with each cause producing its own effect. Here what was originally taken as cause and as effect are both complex. One part of what was taken as cause produces one part of what was taken as effect, and the rest of the original cause produces the rest of the original effect. Thus sun-rays focussed by a burning-glass on a piece of paper saturated with a silver salt will scorch the paper and change the colour of the silver salt. Now it is easy to take the light as the cause and the whole change as the effect. But the proper analysis shows that the true cause is light + heat and the true effect is scorching of paper + change of silver salt. Now both these laws hold independently of each other, and it is only when what is lumped together under the name of light, and treated as a single cause, really has both long and short waves in it, that what is taken as effect, (viz. scorching + change of colour) will happen.

It is important to notice that such an analysis as this cannot always be made even when the cause and the effect are recognisably composed of parts that are separately connected by causal laws. When *A* is connected causally with *B*, and *C* with *D* we are inclined to suppose that *AC* must be followed by *BD*. So strong is this belief that, when the actual sequent is *E*, we think that *E* must really be a complex with *B* and *D* as elements even though *B* and *D* cannot be directly detected in *E*. Now it is true that there are cases in which when *A* is followed by *B* and *C* by *D*, *AC* is followed by *BD*; to such cases the form of analysis (β) applies. But it is not true that if *A* is followed by *B* and *C* by *D*, *AC* must be followed by *BD*. Something—*E*—entirely different from *BD* may follow and we have no right to suppose that *E* is *BD* in disguise unless we can prove this to be so. Here no real analysis is possible.

(γ) The last form of analysis is hypothetical explanation. And this raises the question: Is any observed regularity to be degraded from the rank of a causal law just because human ingenuity can invent hypothetical laws that will account for it? Clearly this is not what is intended. It is meant that the hypothetical laws shall have been verified, *i.e.* rendered probable by the large region of facts that they coordinate. But it may be said: At best they can only be probable whilst many of the regularities which they are supposed to oust from the position of causal laws are practically certain. For instance, it is as certain as anything can be that light is always refracted on passing at an angle to the surface of separation between two media of different density. Yet this is not held to be a causal law. It is, on the other hand, analysed into hypothetical laws which connect terms that are merely assumed *ad hoc.* Thus it is assumed that light, as a physical phenomenon, consists of wave-motions which travel with different velocities through different media, and that these wave-motions when they affect the retinas of normal people produce perceptions of colour which depend on the wave-length. It is then deduced from the laws of wave-motion that there will be the phenomena which we call the refraction of light, and what the laws of those phenomena will be. Now it might be objected that the laws of refraction are all obvious and certain whilst the explanatory hypotheses are all precarious. Why not accept the observed regularity then as an ultimate causal law and drop all these assumed causal laws that are needed when we degrade the observed regularities to mere consequences of really causal laws? The answer to this is that, if we had to deal merely with the refraction of light, it would be most unreasonable to try and get behind the observed regularities in this way. But then phenomena that have such close resemblances to those of refraction as to be equally called optical have also regularities of their own with which the regularities about refraction, if taken as ultimate, seem to have nothing in common. On the other hand the hypothesis which explains the laws of refraction explains without further assumptions those of the remaining optical phenomena. The position then is that, whilst the theory is more complex and less certain *à priori* than any of the regularities which it is supposed to explain taken by themselves, yet by accounting for all these regularities that can be observed to hold it gains a high degree of probability for itself, and its laws are justly regarded as causal rather than the observed regularities.

But it may still very well be objected that there is no difference of principle between the complex regularities which are not held to be causal and those hypotheses which account for them and are held to be causal laws. The answer to this objection turns on the second subject to be discussed in this section, viz. the necessity of causal laws. This vague phrase has several possible meanings which we must separate, but, for the present purpose, the following is the important one. It is believed that a real causal law would assert an unconditional regularity in some sense; in fact, how can you talk of a law when there are exceptions, since a natural law just means what things of a certain kind as a matter of fact do? But as soon as you have reason to believe that a given regularity is analysable into several others there enters a ground for believing that its regularity is only conditional. There is, of course, first of all the ~~residuum of doubt which~~ attaches to any alleged causal sequence and therefore even to the regularities ~~discovered or~~ inferred in the analysis. But, over and above this, there is in any admittedly complex regularity the further doubt as to whether the whole of the elementary sequences can be trusted always to fall together. If any of them fail, then, although each may be truly regular, the complex regularity will break down. As an example of this we may ~~take~~ Kepler's Laws and the Newtonian Planetary Theory. As long as we consider the planets moving steadily and comfortably round the sun Kepler's Laws are all that we need, and they seem to be unconditional regularities. But when we consider the falling of bodies to the earth, the motions of the moon, the Cavendish experiment, etc., we analyse Kepler's Laws and find them to be the results of the First Law of Motion and the Law of Gravitation. These we believe to be unconditional causal laws, and we accept them as we did the results of the hypothetical account of optics because they explain such quantities of seemingly unconnected regularities and apparent irregularities. But, when we have once made this analysis and believe it to be true, our whole attitude towards Kepler's Laws alters. We now know that, instead of being unconditioned regularities, they are merely special cases of the consilience[*] of the new regularities which we believe to be unconditioned. We know, too, what kind of departures to expect from them and where and when to expect them; and, when we look for them we actually find them in the perturbations of one planet by the others.

We are thus led to see how it is that no regularity which there is good reason to believe analysable into several others is accepted as a causal law. And, whatever be the upshot of our enquiry into the meaning and truth of the belief that ultimate causal laws are unconditioned regularities we see that the products of such analyses are at least *less* conditioned than the complex regularities from which they are discovered, and, as such, approach more nearly to the ideal of a causal law.

We are not however even yet in a position to discuss the question of what is meant by a causal law being unconditional and what is meant by its being 'interfered with.' We must first consider certain points connected with the analysis of regularities, since we now see that it is of the products of such analyses that this question will have to be asked and answered. We will then discuss certain points that arise in connexion with the kinds of analysis that I have called (α) and (β).

A cause in the most general sense is a set of successive sets of contemporary events from which other sets of events can be inferred. The question with regard to (α) is: What is the least number of successive sets that is wanted for a complete cause and a complete effect? With regard to (β) the question is how many distinguishable aspects or events are needed in each successive set to constitute a complete cause and a complete effect? It must be remembered that we are already supposed to have found a certain regularity and to be trying to analyse it into truly causal ones. We find that a certain set of sets of events is regularly followed by another set of sets, all within a certain interval of time. What we do in (α) is to make a rearrangement of these successive sets. In virtue of other experiments we know that some that come in the middle of the interval follow regularly on others that come earlier in it, whilst these sequents under certain circumstances have other sequents, as we learn from a fresh set of experiments. The essence of this kind of analysis is that some of the later sets of events in what was formerly taken as the cause are found to be effects of the earlier sets in it, or some of the earlier sets in what was formerly taken as the effect are found to be causes of the later sets in it. Finally the parts of the old supposed cause that are now taken as effects are also part causes of those parts of the old supposed effect which are now taken as causes of the later sets in it. This form of analysis might be called Vertical, because it analyses the successive sets of sets of contemporary events which were taken as whole causes or effects into fewer successive sets which are both causes and effects, but does not trouble about analysing the sets of contemporary events themselves at each moment under consideration.

The kind of analysis mentioned under (β) may be called Horizontal. Here we shall find it necessary to distinguish certain aspects in events. If an event be the occurrence of a sense-quality at a certain moment we must distinguish in that quality (1) the general aspect that makes it the sort of quality that it is (*e.g.* sound or colour); (2) what I shall call 'characteristics,' *i.e.* certain independent variables by which the particularity of the quality is fixed. These are generally few in number, *e.g.* in sound they are pitch, loudness and quality. They generally have intensive magnitude and the range of their possible values is believed to be continuous; (3) the particular values of these variables.

In an Horizontal Analysis of any regularity we have to see whether, out of the sets of contemporary events, successive sets of which constitute 'causes' and 'effects' in our regularity, so long as it is supposed to be unanalysable, we cannot make selections which constitute smaller sets known to be related as causes and effects.

This brings us to the question: Is there any relation that holds generally between the complexity of an effect and the complexity of its cause? This is important as bearing on the question of how complex the real world must be if we hold that it causes our perceptions but do not hold that the objects of those perceptions are the real world. Must every distinguishable event and characteristic in the effect have a differentiation in the cause to correspond to it? Let us begin with events as wholes. If in an effect a (β) analysis can be made so that every distinguishable event is the effect in a different causal law there must of course be at least as much distinction in the cause as there are events in the effect, for the cause of any event must be at least one event. But in general such an analysis is not possible, and then no conclusion can be drawn from the number of distinct events in an effect to that in its cause.

Now let us consider the case of characteristics, which is more fundamental. An event is not fixed till all its characteristics are fixed. Hence the causal laws that determine the occurrence of a definite event must determine all its characteristics too. Let us consider an event like a particular sound with its three characteristics of pitch, loudness and quality all determinate and let us suppose that it is the complete effect of some cause. Must that cause have at least three differentiations? I do not see why it should. We could only conclude that the cause must contain at least one event, but not that no cause can be less complex than its effect either in the number of events in it or the number of variable characteristics. There might perfectly well be an ultimate causal law connecting one event with two characteristics, with four events with three characteristics apiece. I think this view is contrary to the usual opinion or at any rate to the opinion that ought to be held if many applications of causality be valid. I fancy that common-sense would probably use the following argument about characteristics at any rate. Suppose that *A* causes *B* in the sense that *B*'s occurrence at a certain moment is inferable from *A*'s occurrence at a definite period earlier. Suppose that *B* is an event with three characteristics whilst *A* is a single event with only one. Then it will be said: 'Each occurrence of *A* with its characteristic with a certain value is a definite event and as such will allow of the inference of a *B* event. Moreover, if the *B* event is what is inferred from the *A* event there will be one *B* event to each different *A* event and therefore just as many different *B* events as there are different *A* events. But the number of possible *B* events is one for each value of each of the three characteristics that define such events. Hence there will be quantities of *B* events left over uncaused by *A* events.' This argument is invalid, ~~and that~~ for two reasons. (1) It assumes that because all *B* events are alike in being *B*'s, therefore if one *B* event is caused by a particular *A* event, *all B* events must be caused by *A* events. This cannot be proved. At the same time it seems probable and is certainly true in most cases. If it be set aside there is the possibility that only a certain selection of possible *B* events are caused by *A* events, and that all the *B* events which are supposed to be left over when all possible *A* events have been considered are caused by a different set of events which are not *A* events at all. (2) But, setting this suggestion aside, there remains another and a fatal criticism. The argument is only true if the range of values of the characteristics be discontinuous and there be only a finite number of possible values for each. For the argument rests on the assumption that a compact series defined by the values of one variable cannot have as many terms as a series defined by the values of three independent variables whose range of values is compact. Now if the series be really compact this assumption is a sheer error, and with it the argument falls to the ground. It follows then that, even if we grant that the causes of events that only differ by possessing the same characteristics to different degrees, will themselves only differ in this respect, still the cause need not be as differentiated as the effect. (One set of events that are defined by a single characteristic and only differ by the particular values of the latter may, if the values form a compact series, cause any finite number of events with any finite number of characteristics apiece.)*

We can now pass to the question

(2) In what sense are causal laws universal and necessary? We now know that the question must be asked not of any regularity that chances to appear, but only of the products of analysis where ~~that is possible~~. Any of the ordinary regularities which are stated as causal laws in common life are found to have exceptions. And there is no avoiding the fact that a natural law that has exceptions is not as such a natural law but a mere mistake. We say that arsenic is a poison, or more accurately that if a man takes arsenic he will die with certain symptoms; but we know quite well that not all people who

* [illegible]

take arsenic die with those symptoms, since, if they take an emetic soon enough they recover. We might put the matter thus. The set of events that constitutes a cause ~~are~~ is generally separated by a finite interval from that which constitutes its effect. And it is found that the effect depends on what takes place within that interval. Hence what we said was the cause was not the complete cause. On the other hand if you have to take account of what happens at every moment up to that at which what is called the effect happens causal laws are quite useless. Nothing at all is inferred; for there is no moment immediately before that at which the effect happens and our causal law comes to nothing but a mere observation of what has happened at every instant of a finite stretch of time. And again there is a difficulty about the breadth of each momentary state. We have seen that horizontal analyses are not by any means always possible. *A* alone might cause *B*. But then the other events are always happening contemporaneously with *A* and there is no reason why, if *AX* produces *B*, *AY* should do so. [Hence causal laws seem doomed to the barren tautology that if *A* has once been followed by *B* after a time τ, then if the whole state of the universe with *A* in it recurs and all the other states that formerly intervened between *A* and *B* also recur, *B* will happen again after a time τ.]

This is the essence of Bradley's attack on causality; and if it be valid it is fatal. We must therefore examine it carefully. We will begin with the Horizontal difficulty. Bradley's position really amounts to a denial that in any case horizontal analysis is possible. Now we must begin by distinguishing two different kinds of uncertainty which are often confused. They are (*a*) uncertainty whether you have found a certain law and whether it is really true, and (*b*) that the law is uncertain in its operation. A law that is uncertain in its operation is not a law, and there we must leave the matter. But I take it that the argument with which we have to deal is different from this. It would take the form: Every causal law to be of any use must connect only parts of the total states of the universe at various moments. But when the laws are stated in these terms they frequently are found to be false. *A* appears under different circumstances and you infer *B* and *B* does not take place. Now you only can infer the laws that are supposed to connect partial states from observations on what are really total states, and you assume that the part of the state not mentioned in your law is irrelevant. But the constantly proved falsity of your laws proves that this is at any rate often a mistake. What right have you to believe that it is ever anything but a mistake? And, if so, you never can state laws with confidence, for they must always connect partial states to be of any use, and those partial states are always abstractions from what you now admit is quite likely to be relevant.

This objection forgets the fact that causal laws are not merely read off from the book of nature without further trouble, but have to be sought and tested with precisely the object of seeing what is and what is not relevant. Certainly you cannot tell beforehand what is going to be irrelevant, but then the process of discovering causal laws just is the process of discovering what is irrelevant. If I shoot a gun at a person's heart he will generally fall down and be found dead with a hole in his heart. But if he is wearing a metal plate over his heart it is very likely this will not happen. Now certainly there is no *à priori* reason why the wearing of a metal plate should be more relevant than the question of what horse wins the Derby. But an investigation of what actually happens shows that the one is relevant and the other not. And, if we investigate the matter more closely, we can see why the plate is relevant, because we can make a vertical analysis of the process of killing a man by a gunshot, though we cannot see why it should be irrelevant what horse wins the Derby.

The fact is that, so far from it being true that no occurrence in the universe can be known to be irrelevant, whole masses of the universe are shown to be irrelevant every time the law is verified under different circumstances[1]. The real objection is that there do always remain masses of ~~possible~~ events that might be relevant. If you do not take them into consideration you cannot be at all confident that your proposed law is true; but if you do, your law will more and more come to connect unique states of the whole universe and so cease to be a general law. Among the circumstances that cannot be proved to be irrelevant there is one important group for which this fact does not greatly matter. That is the relatively permanent features of the material universe. All the regularities that we observe and from which ultimately all our supposed causal laws are gotten take place in presence of these permanent features, and we cannot tell whether they may not be relevant circumstances in laws which, because of their invariable presence, do not need to mention them. But the same circumstances that make it impossible to tell whether these permanent characteristics be relevant or not make it unimportant to decide. I do not see how we could be sure that arsenic kept for the usual time in a man's stomach would poison him if the planet Mars were annihilated, since all our experiments have been performed in presence of that planet. But then it is so very unlikely that the planet Mars will be annihilated that this possibility is not an objection to a causal law about arsenic poisoning which makes no reference to the planet. We must I think assume in our causal laws a tacit admission that the permanent features of the universe are supposed to be pretty well the same at all times for which they are supposed to hold.

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[1] This is not strictly true. If X happens once in the presence of P and the absence of Q and then again in the absence of P and the presence of Q you cannot decide offhand that P and Q are irrelevant. All you can say is that P is not essential and Q is not essential, but P or Q may be essential.

It is not then the possible relevance of relatively permanent characteristics that troubles us, but that of the constantly changing circumstances under which every new case to which the law is supposed to apply must take place. Now it is quite clear that we are not as a matter of fact reduced to the *impasse* that Bradley suggests. We can foretell certain classes of events with almost absolute certainty without taking into account everything in the universe. For the fact is that, beginning with a few obvious regularities which are seen to hold in spite of any change of circumstances that we can compass, we are led to recognise in the world a system of practically independent causal series and to see that it is very improbable that events of the type *A* should be relevant to causal laws connecting events of the type *B*. At the present stage of our knowledge of the structure of the world we can confidently rule out the possibility that the result of a horse-race should affect the question of whether a bullet aimed at a man's heart will kill him. We know enough about the 'make' of the universe, subject to its hitherto permanent structure remaining permanent, to know the kind of events to which the results of horse-races are and are not relevant. But when we are still in the dark as to the phenomena under investigation we cannot lay down these rules about relevance and irrelevance of certain classes of events to other classes with any confidence, and our position is much more like that which Bradley seems to think it ought to be everywhere. Thus we are practically certain that the strength of illumination is perfectly irrelevant to the motion of a billiard ball after it has been hit with a cue, but it would be radically unscientific to begin by assuming its irrelevance to the telekinetic motion of objects by Eusapia Paladino, supposing that it actually happens. [There is no *à priori* necessity that the universe should be thus divided into causally water-tight compartments, but experience shows that this is largely the case and that, by assuming as a methodological principle that there are such compartments even when they are not immediately obvious, they can be discovered and separated. The universe might have been as unified as some Idealists would wish us to believe, but, by a merciful ordinance of Providence (for which the true scientist will be more thankful than for his 'creation, preservation,' and the other 'blessings of this life') it is not so, and other people beside the Absolute have some chance of disentangling its structure.]

But, granted that things are not as hopeless as Bradley suggests; that we can discover in the universe mutually irrelevant groups of events; and that the permanent characteristics of the universe, whilst their permanence precludes our proving that they are irrelevant, are for the same reason negligible in practice; we are still by no means clear of the general

Bradleian objection. [We still have to admit that within a given isolated group, any cause being given, you cannot with certainty infer an effect because other events of the same group may intervene in the time between cause and effect and so replace the supposed causal law by a new one with a vertically more complex cause. And similarly there is the question of ~~the breadth~~ that is to be given to the cause within that group.]

How then must a causal law be stated for the events of a group when it is admitted that in general a finite time elapses between cause and effect and that your law is useless unless the momentary states mentioned are less than the total momentary state of the group? The two difficulties are closely connected. Suppose that the regularity is not found to be analysable vertically but that the intervening events coalesce with the ones that have gone before to give a new cause with a new causal law. Then, if the general law of causality be true, those intervening effects could have been ~~predicted.~~ ~~But~~ in order to predict them we should have needed a wider knowledge of the earlier states in the total isolated group than we actually had. In the most general case we should need for a completely certain prediction (i) a wider knowledge of the momentary events in the group that are contemporary with what we at first took to be the cause, (ii) a knowledge of the causal law by which we could predict ~~from them~~ the intermediate states that we had formerly left out of consideration, and (iii) a knowledge of the new causal law connecting the new cause, which consists of what was originally taken as cause + the intervening events which (i) and (ii) have enabled us to foresee, with the new and actual effect. And it is clear that the causal law mentioned in (ii) will be open to the same criticisms as the original one to supplement which it was introduced. Thus it seems that you will never be able to stop till you have taken in all the events at all the moments under consideration in the group, and then you will no longer have inference but mere statement.

I think that so long as we insist that causal laws shall be capable of giving us certainties about the occurrence of events this reasoning is fatal to them. But I think that they can be defended if we hold that they give us certainties about the probabilities of the occurrence of events. The line of argument that I wish to suggest is that a causal law is not properly of the form 'the occurrence of *X* at *t* makes it certain that *Y* will occur at $t+\tau$ no matter what moment *t* may be'—which amounts to saying no matter what other events may be contemporary with *X*. For this I would substitute: 'It is certain that the occurrence of *X* at any moment increases the probability of *Y*'s occurrence at a moment τ later over what it would have been if *X* had not happened.' This is the suggestion that we must now consider for a moment.

Of any event that can be proposed to us as happening at a certain moment it is evident that it is eternally true that it will happen then or that it will not. By this I mean that the alternative is settled one way or the other eternally whether we know or suspect or do not know or suspect which way it is settled. It is also evident that this amount of certainty has nothing to do with causal laws, although it sometimes seems to be thought that it has. It would be equally true that of the two propositions: I shall die on the 22nd of September, 1913 and I shall not die on the 22nd of September, 1913, one definite one is true, without reference to time, whether there were causal laws in the universe or not. Hence causal laws and probabilities must have to do with something beside the truth or falsehood of propositions. Again, one of the propositions being true and the other being false independently of our knowledge, we may suspect that the probability of the two will have something to do with knowledge. The first point on which to be clear will be just what the connexion is between truth, probability and knowledge. A ~~proposition does~~ not have probability, though it does have truth or falsehood, when taken in complete abstraction from other propositions. Probability arises from the fact that propositions are so related that the knowledge of certain ones alters the expectation that we ought to have of the truth of one of the alternatives, of which, as we saw, we know *à priori* that one definite one is eternally true, but not which one it is.

Now, since the question of knowledge thus enters, we may ask whether the probability of a given proposition depends on the other propositions that have this relation whether they be known or not or on the *knowledge* of these propositions. If we take the latter alternative it might seem that the probability of a given proposition would become purely subjective, which no one believes to be the case. But this is not necessarily so. You might say that 'the probability of the proposition *p*' is an elliptical phrase, which taken by itself has no meaning or only a conventional meaning. Thus it might be held that the proper statement would always be 'the probability of *p* given the knowledge of *q*, *r*, etc.'; and that by the phrase 'the probability of *p*' used by itself all that was meant was 'the probability of *p* given that amount of knowledge that may be assumed in all sane people or in all the people to whom my remarks are addressed.'

Would this make probability objective in the sense that is needed? When A and B say 'the probability of p is α' and 'the probability of p is β' respectively; they might both be correct on this theory. For one might mean the probability given the knowledge of q, and the other the probability given the knowledge of r. But would this give us any more than the sort of statement that 'sugar in tea is pleasant' gives; where apparent contradictions are avoided by substituting for this elliptical phrase: A finds sugar in tea pleasant and B finds it unpleasant? This is not as much objectivity as is wanted for probability-propositions, because it is held that people can make mistakes about the probability of propositions, whilst they can hardly do so about their likes and dislikes.

But this objection could easily be met on the view under discussion. It might be said, whilst A and B may certainly both be right in their judgments of probability, it is equally true that they may both be wrong. This may happen in two ways. When A says 'the probability of p is α' he means the probability of p relative to his present knowledge. Now he might think that the only relevant bit of knowledge that he had was q whilst really he knew r which was also relevant. Or he might be right in supposing that the only relevant piece of knowledge that he had was q, but wrong in his estimate of the probability of p given q. It is tolerably obvious that the two kinds of mistake come to the same. To suppose that r is irrelevant is just to make an erroneous judgment about the probability of p given r.

But, if this defence of the possibility of making mistakes about probability, although the correct probability is always ~~that~~ relative to the knowledge of the person who makes the probability-judgment, is to be tenable, we must clearly assume that the knowledge of the person in question, as to how far what he knows is relevant to the probability, is not a part of the knowledge that has to be taken into consideration in saying what is the true probability relative to his state of knowledge. It is clear in fact that the opposite view would lead to a vicious circle fallacy; since then the true probability of *p* relative to a person's knowledge would be the probability relative—among other things—to the knowledge of the true probability. Hence if this position is to be held at all we must put it in the form that: to every amount of knowledge there corresponds a true probability of any proposition relative to that knowledge, but the knowledge of what the true probability is, cannot be one of the data on which it depends.

But there remains a serious objection to it. Everyone will admit that we ought to prefer a probability calculated on a wider to one calculated on a narrower basis, even though the man who only had the narrower basis of knowledge had made his calculations properly. But if probability is to be reckoned relatively to people's knowledge of propositions and not to the propositions themselves it is difficult to attach any meaning to this preference. You cannot get over the difficulty by saying that the knowledge that *A* has given the probability *α* to *p* relative to his knowledge of *q* and *B* has given the value *β* to *p* relative to his knowledge of *q* and *r* is itself a relevant piece of knowledge for you and one relative to which the probability of *p* is *β* and not *α*. For, whatever else this may or may not explain, it does not explain the fact that *A* himself will want to increase his knowledge as much as he can before he is confident of the value of the probability of *p*. On the present theory the only doubt that *A* ought to have is whether he has ascribed the right probability relative to what he does know. If so, his probability is correct; and *B*'s probability relative to a wider basis of knowledge is no better than it. But no one really does believe this to be the case. We feel that, although *A* and *B* have both calculated rightly and attributed the right weight to what they know, *B*'s value of the probability of *p* is in some sense preferable to *A*'s; and this belief seems incompatible with the statement that you can only state probabilities relative to what is known by someone.

Hence we must adopt a different theory as to the relation between probability, truth, and knowledge. We shall still say that a proposition *p* has not a probability taken in abstraction, but that the probability depends on other propositions. But we must now say that it depends on the propositions and not on the knowledge of them. The state of affairs then is this. To every proposition stating the occurrence of an event at a certain moment there are other propositions making similar statements and the probability of the first depends on the latter whether they are known or not. We must not suppose that even though all these propositions were known the proposition in question is one about which we ought to be ~~certain~~ one way or the other. *The* probability of *p* is that value which persons who knew all these other propositions would ascribe to the probability of *p* if they made their calculations properly. What then are we to say about the probabilities rightly calculated by persons who do not know all the relevant propositions? I think the following is what should be said. When *A* says that the probability of *p* given *q* is *a* he may be perfectly correct; but we must not suppose that when he speaks of '*the* probability of *p*' he means the probability of *p* given *q*, even if *q* is the only relevant proposition that he knows. For there may be other relevant propositions that he does not know, and *the* probability of *p* is that relative to all these + *q*. But supposing that *q* is the only relevant knowledge that he possesses, whilst someone else—*B*—has the knowledge of *r* which is relevant, *B*'s value of the probability (if properly calculated) is to be preferred, because he is nearer the position of knowledge in which *the* probability could be calculated, viz., that in which the whole of the relevant propositions are known.

This however is only a rough first approximation to a proper theory. This is easily seen from the following considerations. It may often happen that when *p* only is known the probability of *X* happening is very ~~high. As time goes~~ on we may learn the additional relevant proposition *q* and the probability of *X* relative to *pq* may be quite small. Yet *X* may actually happen. In this case we seem to have gone wrong by taking in more relevant information. For instance *p* may be the proposition that someone has taken poison, *X* may be his death within a certain time. The probability of *X* given *p* alone will be large. Suppose we learn *q*—that he has taken an emetic—then the probability of *X* given *pq* is pretty small. Yet he may actually die. So the probability relative to the wider range of relevant propositions is not always in practice a safer guide than that relative to a narrower selection. I do not profess to offer a perfectly satisfactory theory of the relations between knowledge, probability and truth. At present the best suggestion I can make is the following, and I offer it for what it is worth.

Every proposition is eternally either true or false. The law of causality is perhaps the assertion that to every true proposition that asserts the happening of an event at a time there is a set of relevant true propositions such that relative to the whole of them the probability of the event happening is 1. (In this case an uncaused event like a free volition on the indeterminist view would be one that actually happens, but whose probability is not 1 relative to any set of other true propositions.) The part played by our minds is a selective one due to our partial ignorance. The result is that we know selections only of all the propositions that are relevant to the occurrence of any given event. Different people know different selections at the same time, and the same person knows different selections at different times. Now what we want for practical purposes of prediction is that when an event is actually going to happen its probability relative to the relevant propositions that we know shall be high, and when it is actually not going to happen the corresponding probability shall be low.

Now, while we must admit that with any selection less than the whole of the relevant propositions the probability may be low though the event is actually going to happen, or high though it is actually not going to happen, still we shall less often go wrong if we expect an event to happen when its probability is high relative to a larger selection than if we base our expectations on probabilities relative to smaller selections. For the larger the selection the nearer it approaches to that group of propositions relative to which (if the law of causation be true) the probability of an event that actually happens happening is 1. Very often we may go wrong by following this injunction to maximise our selection, for we may acquire our knowledge in such an unfortunate order that we start with a set of propositions that make what is true very probable and then keep on acquiring knowledge that decreases the probability whilst we fail to reach at all the few remaining ones that would once more make it high. For all that, the maxim remains the most reasonable one to follow; just as it is reasonable to argue logically, although you might come to wrong conclusions by arguing correctly from false or insufficient premises and to right ones if you had only simply converted enough *A* propositions and had enough undistributed middles. Very often the worst that will befall you in following it will be that the probability of a true proposition which, relative to a smaller selection, was nearly 1, hovers about ½ relative to the wider selection that you can reach. But this will lead to no error, for you will simply not feel justified in predicting at all. You will then miss some truth, but you will have no expectation disappointed.

I think that we can now see our way to a proper statement of a causal law and to the answer to Bradley's objections. A causal law subsists between two sets of events when they are so related that the proposition asserting the occurrence of one of the first set strengthens the probability of the occurrence of one of the second set. Now it is to be noted that the *strengthening* is the same whatever other events may happen; the only point is that the other events may weaken more than the given one strengthens. Thus let us take the proposition: Smith will die in the next quarter-of-an-hour—it being assumed that this is asserted at some definite moment so that it is definite with respect to time and is therefore a genuine proposition. Then this proposition is eternally true or false whether there be causal laws in the universe or not. But, if there be causal laws, there may be, and, if the Law of Causality be true, there will be propositions asserting the occurrence of certain other events and such that the original proposition has a definite real probability depending upon these whether they be known or not. Suppose, for instance, that Smith has taken arsenic within the last few minutes. Then this proposition largely increases the probability over what it would be if this were not true. But it does not follow from this that the probability is large, because another proposition that may be true is that he will take an emetic immediately. But the point to notice is that it remains true in spite of this that the proposition that he has taken arsenic does strengthen the probability that he will die within the next half-hour whether other propositions be true that weaken it or not. Causal laws then as laws about the strengthening of the probability of the occurrence of one event by the occurrence of another remain on ~~any~~ view. The only question is whether we can go further than this and whether it is ever reasonable to suppose that we have actually arrived at the true probability of the occurrence of an event.

In our actual application of causal laws we do not attempt to calculate numerical probabilities. What we want to know is whether an event is very probable or very improbable. Bradley's criticism assumes that we expect causal laws to give us absolute certainty; but no scientist would demand this and the laws cannot be defended if such a demand is made. The question then for us is: Are we ever justified in holding, on the data that we can have, that the occurrence of an event is practically certain? Consider what this means. It means: Granted that relative to the relevant propositions that we know, other than the knowledge that all our knowledge is limited, the proposition *p* is very probable, have we any right to hold that its probability relative to *all* actually relevant propositions is 1? It seems to me clear that we often have a good right to do so. Our right depends on how far we are justified in holding that the propositions that we do not know, if there be any that are relevant at all, are either few or have very little influence on the probability compared with those which we do know. Now we have already seen that we soon learn to cast aside whole masses of the universe as irrelevant to certain aspects of it. Thus the field within which relevant data are likely to lie is often a very restricted one as to breadth. And within this it is often possible to arrange so that the occurrence of any relevant events other than those which we have taken into account is most unlikely. If Smith in the example be locked up in a room and plenty of arsenic administered I think he will exhibit unreasonable optimism if he finds any comfort in Mr Bradley's argument from the complication of the universe.

We may hold then, I think, that Bradley's argument does not apply in our world to causal laws when they are stated in terms of the strengthening of probabilities. Causal laws it may be admitted at once do not give us complete certainty, but this does not prevent their being laws, and it does not conflict with any belief that anybody who uses them has held about them. We can pass then to the last objection to Causality with which we shall have to deal, viz.

The alleged Antinomy of a First Cause. We shall not have to spend much time with this venerable friend, who, like most elderly antinomies, has the misfortune to be lame in one leg. The argument for the thesis is that causation claims to explain events, and, as it only does so by referring to other events which need just as much explanation, it involves a vicious infinite regress, unless we assume a first cause. The argument depends on using 'explanation' in the sense of proof. The propositions of Euclid follow from the definitions and axioms. The axioms are also propositions, but they do not need a proof because they are supposed to be self-evident. Similarly it was thought that causal series must end with an uncaused cause. But difficulties at once break out in such an analogy. The axioms of Euclid differed from the propositions by the fact that they were self-evident whilst the propositions were not. But the first cause was either an event or not. In general it was taken to be the first event in a certain substance, viz. God. But this event, unlike the axioms of Euclid, had no distinguishing characteristic, like that of self-evidence in their case, to differentiate it from the other events which were supposed to need causes, as self-evidence differentiated axioms from propositions that needed proof. Hence *it* required a cause as much as any other event, and this must be events in the same or a different substance, and so the old regress broke out again. It was impossible, as Schopenhauer put it, to 'take the Cosmological Argument as if it were a cab and drop it when it had taken one as far as one wished to go.' Thus the antinomy is weak on the side of the thesis. The analogy of causal explanations with logical proofs which started the regress breaks down at just the point at which we wanted it to stop the backward journey.

On our view of causation, however, there is no reason even to begin the regress, because we do not hold that there is any analogy between proof and causation. The possibility of causal laws merely means that there is a certain amount of unity in the world, which, on further investigation, is found to take the form of a set of more or less isolated groups within which laws hold. In virtue of this fact the world is not a perfect chaos in which nothing can be legitimately expected at one time rather than another, but it is subject to certain laws such that the happening of one event or set of events, when known, has a legitimate influence on our expectation of the occurrence of other events. The fact that our knowledge of the occurrence of the events *B* strengthens our expectation of the occurrence of the events *C* and that there were events *A* which had they been known to have happened would have strengthened our expectation of the events *B* and so on presents no vicious regress. In fact there is no real analogy between causal explanation and logical proof. The only sense in which causal laws explain is that they simplify. They do not show us why an event happens in terms of some event or law that is self-evident, for one event has no distinction from another to correspond to degrees of self-evidence among propositions. What they do tell us is that we can hope to know with some certainty what will happen where and when we cannot have or do not wish to have direct experience. It is in this sense that it is right to insist with Mach that their value is an economic one, whilst at the same time we definitely take our stand against the Pragmatists and deny (*a*) that this is what is meant by their truth, and (*b*) that it is a test of their truth. It is because it is true that they are of a certain definite nature that they are of economic value to thought, and it is because predictions made by them are found to be verified by experience that they are believed to be true. In this sense of explanation, which is the only one that causal laws will bear, the Kantian antinomy leaves them untouched.

But it may be said: This is no doubt true of your causal laws, but what about your axiom that a system that has been quiescent for a finite time can only have its emergence from quiescence explained by a causal ~~law transeunt~~ to the system in question? Suppose we accept the axiom as I have done, what will happen? The sole difficulty that can arise will be over the beginning of change in the universe. If this axiom be true it is clear that the whole universe (meaning the world + God, if there be one) can never have been completely quiescent. For it certainly is not so now, and if it ever had been it would only have got out of that state through a transeunt causal law which, with respect to the whole universe, is a contradiction in terms. But really there is no difficulty about this whatever. Why should the whole universe ever have been quiescent for a finite time? Apart from the axiom, the question is a perfectly open one and with it we must decide that it never has been quiescent for a finite time. The only argument that could be brought against this view must be based on the supposed difficulties of infinity and continuity. But, since these difficulties are at an end, arguments based on them alone may be relegated to that cave in *Pilgrim's Progress* where Giants Pope and Pagan mumble the bones of their past victims and growl at the passers-by whom they can no longer hurt[1].

We have now discussed all the classical objections to causal laws, and have tried to show that none of them are of weight against the view of such laws as which we have offered and which suffices for natural science. It only remains to close this chapter with a word about the Law of Causality as opposed to particular laws of Causation. The Law of Causality is that every event has a cause and it is often supposed to be an *à priori* truth. Now we have admitted as an *à priori* truth the law that a system that has been quiescent for a finite time can only be set in change by a causal process transeunt to itself. The certainty of this law may cover two certainties, (*a*) that such a system will only be set in motion by causes, and (*b*) that those causes will be partly transeunt to the system. Now are (*a*) and (*b*) both equally self-evident? It seems to me that (*b*) is self-evident; on the other hand (*a*) is a particular case of the general Law of Causality that every event has a cause. Of course the particular case might well be self-evident without the general law being so too. That every event has a cause means on our theory that to every true proposition asserting the occurrence of an event at any given time there is a number of true propositions asserting the occurrence of other events at different (and perhaps, to be in accord with tradition, we should add earlier) times such that relative to this set the probability of the event's occurrence is 1. This proposition does not seem to me self-evident, nor do I know of any means of proving it. At the same time it obviously cannot be disproved and it is advantageous to assume it as a methodological postulate. So far we have found that by assuming it even in the most unpromising cases we can find laws such as it suggests. But we never know when it may break down, and some persons hold that it does so over volition. I do not know that there is nearly such good reason to think that it breaks down over volition as over the occurrence of earthquakes, but as some people take an innocent pleasure in believing that their volitions are not even theoretically predictable, it will be pleasant not to have to rob them of that harmless opinion.

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[1] It must be noted that the fact that the universe can never have been quiescent for a finite time is perfectly compatible with its having a beginning in time. *The universe began at t* means that nothing existed at any moment earlier than *t*, whilst at *t* and all later moments up to now something has existed. *The universe has never been quiescent for a finite time* means that if *t* and *t'* be any two moments there is always an intermediate moment such that what existed at it differs either from what existed at *t* or from what existed at *t'*.

Is the particular case of this supposed law, which we see forms a part of the axiom about causality, self-evident? That takes the form that whenever a system that has been quiescent for a finite time ceases to be so there is an event or events so related to the new one in the system that the knowledge of them strengthens the probability of the latter. I am inclined to believe that this is self-evident. If I enter a room and find that a chair which was by the fire is now by the window I invariably hold that if I had been present I might have had knowledge of events which would have strengthened the probability of this change. In fact, apart from the knowledge that such events may have happened in my absence, or, to put it more strongly, with the certainty that they have not happened, the change of position of the chair has no probability at all.

CHAPTER III

ON PHENOMENALISM[1]

BEFORE coming to the chapter on the Causal Theory of Perception and its effects on our beliefs in the reality of the objects of our perceptions, I propose to devote a short space to the discussion of Phenomenalism.

No apology is needed for discussing this theory somewhere in any essay which deals with the question of the information, if any, that perception can give us about reality. And when this question is raised with particular reference to the philosophical position of the truths of natural science, such a discussion is essential in view of the fact that Mach and his school are phenomenalists, and hold that phenomenalism is the philosophic theory that is best suited to be the basis for physics. The only preliminary point that does call for some explanation is why I should discuss phenomenalism here rather than after the chapter on the Causal Theory of Perception. It will be said that it is arguments based on the causal theory that chiefly undermine naïf realism, and that it would be a more reasonable order to discuss phenomenalism as an alternative when the difficulties of naïf realism were becoming insuperable than here, where they have hardly fairly begun.

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[1] It must be understood that in this chapter I am not discussing arguments for phenomenalism—so far as I am aware—that any phenomenalist uses. The average phenomenalist bases his position on the kind of considerations that Mr Moore overthrew in his *Refutation of Idealism.* For the purposes of the philosophy of science Mach is the most important phenomenalist. But he has no positive arguments for his position that are worth discussing. He is vitiated by the fallacy that Mr Moore overthrew, and it seems that his main reason for holding the doctrine is that he supposes—for some inscrutable reason—that it is scientific and 'economic' to a preëminent degree. Anyhow he has now retired behind the formula "Es giebt keine Machschen Philosophie." I have therefore tried to invent the argument which a philosophical phenomenalist might reasonably use, and to criticise it.

We may anticipate the results of the next chapter so far as to agree that the main difficulties of naïf realism do spring from the causal account of perception; but we may still defend the order that we have adopted. For we shall try to show in the present chapter that causal arguments that refute naïf realism cannot be used to support phenomenalism. Moreover, the main problem about phenomenalism from the point of view of the philosophy of natural science is to be found in the question of its relation to causal laws, and so there is good reason for discussing it directly after we have finished with Causality. I shall therefore proceed to discuss phenomenalism without further apology.

First of all, what precisely is meant by phenomenalism? In the sense in which we propose to discuss it it is the theory about the reality of the world with which we come in contact in perception which is diametrically opposite to that of naïf realism. It holds, not merely that the objects of all our perceptions exist only when they are perceived, but also that there are no permanent real things with laws of their own that cause these perceptions and in some measure resemble their objects. The laws of science, stated in terms of such supposed realities and their states, are for it mere transcriptions of laws connecting the perceptions that people actually have, and these perceptions and their laws are all that we can hope to make the objects of science.

Phenomenalism, unlike naïf realism, is a position that needs proof. Every man is a realist except in so far as experience and reflexion force him away from that position. But nobody becomes a phenomenalist except by argument. Nor is phenomenalism the position at which we naturally arrive on leaving naïf realism[1]. As soon as the average man is forced away from naïf realism at any point he always assumes that, in the case of every characteristic that does not raise some special difficulty, he perceives the real, and that events in that real cause the perception of that characteristic which he now has to believe to be appearance. What, then, are the arguments for phenomenalism?

Clearly it has to refute both naïf realism and the modified form of realism that is put in its place. We have already discussed the arguments against naïf realism that are independent of causality, and seen that most of them have very little weight. And the arguments that remain to be discussed in the next chapter from the relativity of perceptions to an organ will not really prove phenomenalism. The argument is a little complicated, and we had better put it formally, in order to avoid all chance of error. Let p be the proposition 'phenomenalism is true.' Let q be the proposition that the objects of our perceptions depend on the structure of our organs. Can we prove p from this, i.e. can we at the same time assert q and $q \supset p$? In the first place, if phenomenalism be true, our perceptions cannot depend on the permanent structure of our organs, for they will have no permanent structure. They exist when somebody perceives them, but not otherwise. Hence, unless you can be sure that, e.g., somebody always perceives your eye when you perceive a colour, there is no truth in the statement that what you perceive by sight depends on your eye and its structure. Every time you perceive a colour when no one does perceive your eye, you have a perception of colour which does not depend on the existence of an organ and its structure. Hence we can assert the proposition $p \supset \sim q$. Taking this together with $q \supset p$, we get $q \supset \sim q$. But $q \supset \sim q . \supset . \sim q$. Hence we reach the conclusion that $q \supset p . \supset . \sim q$. Thus to assert *both* $q \supset p$ and q would involve the assertion of both q and $\sim q$. You cannot do this, and therefore you cannot prove p from the argument. It is true that Berkeley, whose argument is properly phenomenalistic, is so shocked at this result that he introduces God either—for the point is uncertain—to perceive your eye when no one else does, or to be a permanent cause which can make people perceive an eye whenever someone else perceives a colour. But either alternative is a departure from pure phenomenalism. The first alternative is ridiculous, unless there be other grounds for believing in the existence of God. Taken as an argument for God, the position might be stated as follows: 'I have produced a theory about the unreality of the objects of our perceptions which is intrinsically so contrary to what people generally believe that it needs powerful proofs. What would be a strong proof, if it were consistent both with known facts and with my theory, is unfortunately inconsistent with them. It would cease to be so if I introduced a new fact, viz. a percipient God. Therefore it is obvious that such a God must exist.' The second alternative takes us away from phenomenalism to a form of idealism, for it now holds that our perceptions have permanent causes common to all of us under like circumstances; but it goes beyond this by supposing that these causes are to be found in the volition of a single person whom it identifies with the God of theology. The first step takes us from phenomenalism to a form of realism, the second to a form of idealism.

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[1] As with naïf realism so with phenomenalism there is no room for appearance, and the plain man does soon distinguish appearance and reality.

We may then, I think, agree that no arguments based on relativity of perception to an organ can by themselves legitimately lead to the proposition that all that we ever perceive is appearance, and that there corresponds nothing permanent in the real to the objects that we time and again perceive, and that common-sense takes to be relatively permanent realities. I do not, of course, wish to deny at this stage that the propositions about relativity to an organ could be stated in a roundabout way in terms of phenomenalism. I only want to show that they could not, when so stated, be consistently made into an argument to prove the truth of phenomenalism in general.

There is, in fact, no direct argument for phenomenalism that can make any claim on us. The doctrine, if held at all, can only be held reasonably on some such grounds as the following: Suppose it were found that naïf realism could not be maintained, and that the causal view that is substituted for it leaves us in complete agnosticism about the real, then the cry might well arise: Why not drop all reference to the real and state everything in terms of perceptions and the laws of their connexion? To this question the answer must be: Either you do not intend to attempt to find any causal laws that will tell us what perceptions to expect or you do. In the former case you must remember that your theory is less well off than the one that boldly assumes real causes of our perceptions rather like their objects, and assumes that they obey certain laws. For these assumptions do account for a good many of the perceptions that we have, and there is no reason to suppose that there is any *à priori* objection to making them. In the latter case, if you are to keep to pure phenomenalism you will have to account for present perceptions by causal laws that bring in no data beside other perceptions. And it may certainly be questioned whether you will be able to do this. If you bring into your laws anything like 'possible perceptions,' or 'perceptions beneath the threshold of consciousness,' you have, however, left pure phenomenalism. For you are now assuming the existence of something that is not, and never has been, the object of a direct awareness, and that is precisely what the ordinary scientist does in his assumptions about the real world. The question will then merely be (*a*) whether his assumption that the real world is on the whole very much like the objects that he perceives is *à priori* less probable than yours, and (*b*) whether your theory can explain the occurrence of the perceptions that we actually have as well as the rival theory.

I think it is perfectly clear that an absolutely pure phenomenalism that wishes to explain and anticipate our perceptions can be ruled out of court. We will suppose that it is allowed to assume present perceptions and those that it can remember. It is quite clear that with these alone there are no causal laws possible that will account for the perceptions that we may expect to have anything like as well as the assumptions which science makes will do. To make such laws possible we shall certainly have to take into account the perceptions that other people have, that we and they might have, and those that we have had but have forgotten. The question is whether the processes by which the phenomenalist—who has now ceased to be a pure phenomenalist—arrives at his beliefs in all these other perceptions would not equally justify the plain man's assumption of a real world more or less like what he perceives.

We will consider, then, the phenomenalist's answer to the two questions proposed to him on the last page.

(*a*) Why should it be held to be *à priori* more probable that what is real is perceptions than that it is something like the objects of our perceptions? To this the phenomenalist would answer: Everyone has perceptions, and perceptions at least must certainly exist whether they themselves are objects of direct awareness or not. On the other hand, the objects of our perceptions are clearly only known to exist in the relation of being perceived. What right, then, have you to suppose that they could exist out of this relation? Thus the argument is that in assuming other perceptions as the real causes of our present ones we are only assuming that the real consists of what we know on other grounds must be capable of existing unperceived; whilst in assuming that it is like the objects of our perceptions, we are assuming that what exists unperceived is like that which is clearly only experienced as perceived, and cannot be proved independently to exist in any other state.

We must consider both sides of this argument carefully. We must keep separate the two distinctions of conscious and unconscious perceptions, and of perceptions which are, and those which are not, reflected upon. These two differences are often confused. Let us begin with that between those that are and those that are not reflected upon. It is quite clear that when I perceive a tree or any of the objects of ordinary life I do not generally reflect upon this perception and say that I know that I perceive a tree[1].

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[1] This does not of course prove that I am not as a matter of fact directly aware of my perception when I have it, but merely that I *do* not make a certain judgment which I *could* not make unless I were directly aware of the perception.

And it is generally considered that the opposite view would involve an infinite regress in knowledge that is psychologically impossible. On the other hand, my awareness of the tree, whether reflected upon or not, would be called a 'conscious awareness,' as against the so-called unconscious perceptions that I am supposed to have by Leibniz, or believers in the sub-liminal self, when I hear the roaring of the waves. These are supposed to consist of perceptions of the numberless little noises due to the rolling of the separate stones, and they are supposed to differ from any perception like that of the tree in a definite way which would perhaps best be described as a difference of intensive magnitude. Now, it is clear that the distinction between perceptions that are and those that are not reflected upon is a valid one, and can be witnessed by introspection, but there is much more doubt whether the distinction between conscious and unconscious perceptions is valid. Unconscious perceptions have often been introduced where either there was no need to assume any event in the brain or the mind, or where all that was needed was a persistent state of the brain or the mind, or both, which alone does not produce or constitute a perception, but in company with other such states, or under new bodily or mental conditions is capable of giving rise to a perception. But there is no more reason for calling a psychical state of this kind an unconscious perception than for calling a match-box an unlighted bonfire. Similarly if we grant that at a given moment there may be unanalysed detail in the object of a perception which attention can discover we do not assume any but conscious perceptions. Before it was discovered it was part of the object of a total conscious perception; afterwards it forms the objects of several new conscious perceptions. Perceptions, then, are either conscious and like our perceptions of trees and chairs, or there is no reason for calling them perceptions at all. And it is certainly not essential to the existence of perceptions that they should be reflected upon.

Hence, to be plausible, the phenomenalist position has to be stated as follows. The assumption that the real causes of our perceptions are perceptions that we have had and have forgotten (i.e. on which we have ceased to be able to reflect) or are perceptions in other people (i.e. perceptions on which we never could have reflected) is so far more probable than any other alternative *à priori* in that we know that perceptions can and do exist unreflected upon, whilst we do not know that the objects of those perceptions, or anything like them, exist unperceived.

Before discussing this argument in its present form we must say a word in explanation of the second half of it. This is the argument that, since the objects of our perceptions are clearly only known to exist when perceived, it is a greater assumption to suppose that the unknown real causes of our perceptions are like their objects than that they are other perceptions, since we have now seen that other perceptions are able to exist unreflected upon. This does not, of course, mean that when I perceive a tree I perceive it as something perceived by me, for this would be just to deny the true assertion of the other part of the argument that perceptions can and do exist unreflected upon. What it means is that a thing is only directly known to exist while it is actually an object of perception, whether, as a matter of fact, we did or did not reflect at the moment of perceiving it that this was the case. The point, then, is that, whilst there is no ground independent of the success with which the assumption meets in accounting for the occurrence of our perceptions)which makes it necessary to suppose that the objects of perceptions, or anything like them, exists unperceived, it is certain that perceptions can and do exist unreflected upon. The assumption that the unknown real causes of our perceptions are other perceptions that are unreflected upon by us is therefore *à priori* a more probable or a less improbable assumption than that they are like the objects of our perceptions.

Now that we have stated this argument as fairly as possible, there are two criticisms to be made upon it, one from each side. From the side of the argument that perceptions can certainly exist unreflected upon, we must ask whether the sense in which this is true is the sense in which it will make the phenomenalist assumption of forgotten perceptions and perceptions in other people as the real causes of our perceptions more probable *à priori* than the rival assumption. The phenomenalist wants to be able to assume forgotten perceptions and perceptions in other people. He argues that these are just perceptions that are not reflected upon by the person who assumes them, and that, since it is known that such can exist, the assumption of them is *à priori* the most probable one that can be made about the nature of the real. I think that this argument loses most of its weight when we examine a little more carefully the meaning of reflexion. We shall see, in fact, that, if the assumption of forgotten states of mind in oneself is *à priori* slightly more probable than that of a real world like the objects of our perception, the assumption of perceptions in other people—which is certainly necessary for phenomenalism even more than for other views—is not more probable.

When we say that it is certain that perceptions can and do exist unreflected upon, we mean that we can now remember to have had many perceptions of which we can also remember that at the time at which we had them we did not think or say to ourselves: 'I have now a perception of *X*.' This, it is to be noted, involves memory. I am directly aware now of the perception that I had some time ago, and I am directly aware that I did not at the moment at which I had the perception have the sort of experience that enables me to say, 'I have such and such a perception.' Such a proposition would, of course, have been true; but I did not then have, as I do now, that direct awareness of my perception at that time which would have enabled me to assert it. You can only show that you have perceptions of which you are not directly aware when you have them by becoming directly aware of them later, and not by the alleged infinite regress that accompanies the opposite view. That regress only applies to the assertion that you cannot know without knowing that you know, and knowing that you know that you know, and so on. But it certainly does not disprove the possibility that every perception might be accompanied by a coexistent awareness of it. We only discover that this is not the case by becoming directly aware through memory of a past perception, and also of the fact that there was no awareness of that perception contemporary with its occurrence[1].

We can now see that phenomenalism has just as much and just as little right to assume other minds as common-sense and science have to assume that the causes of our perceptions are like their objects in general character. It is true that we know directly that perceptions have existed at moments when we were not directly aware of them. But it is also clear that the only perceptions of which we know this are those of which we have been directly aware at some time. Now, the perceptions of other people just are perceptions of which we never can be directly aware. Hence the passage from perceptions in us of which we were not aware when they existed to the assumption of perceptions in other people is not a mere assumption of more of the same kind, but an assumption that perceptions of which we can never be directly aware exist, whilst all that we know directly is that perceptions of which we are sometimes, but not always, aware exist. For all that direct experience can tell us, it might be the case that the only perceptions that can exist are those of which we are sometimes directly aware. Of course, I do not use this as an argument to show that we make a mistake in assuming perceptions in ourselves and others that are never the objects of direct awareness, but merely to show that the phenomenalist who assumes perceptions of which he can never *ex hypothesi* be directly aware, makes a jump just as much as the man who assumes realities like the objects of his perceptions but which are not perceived. To be strictly fair, however, we must grant that the phenomenalist's jump from solipsism is not as great as that of the man who holds that the real in its general character resembles the objects of his perceptions. To assume that there exist perceptions of which I am never directly aware when I know that there are perceptions of which I am very rarely aware, and which exist when I am not aware of them, is undoubtedly a less assumption than to suppose that something like that which I can never know to exist except as an object of perception exists unperceived.

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[1] How far memory can be taken to prove a negative on such a point I should hesitate to dogmatise.

And in the matter of assuming forgotten perceptions of his own the phenomenalist's position is still less open to cavil. A forgotten perception is one of which I believe that I could at one time have been directly aware, but of which I can not at the present be directly aware. Now, of course, it is true that the only perceptions of which the phenomenalist can be immediately certain that they existed when he was not directly aware of them are those of which he can be directly aware when he makes the judgment, and *ex hypothesi* perceptions that he has forgotten are not in that position. So that there is a jump into a slightly different territory when I assume perceptions of which I am no longer able to be directly aware. But the jump is less in this case than that which takes the phenomenalist out of solipsism. There he started with perceptions of which he was immediately certain that they existed when he was not aware of them, but could only be thus certain because he could be aware of them when he made the judgment, and assumed perceptions of which he never could be aware. Here he starts from the same set of perceptions and assumes others which only differ from them by the fact that he can no longer be aware of them at will, although he believes that he could have been aware of them at some time.

We must now consider from the other side what is to be said about the phenomenalistic argument that its assumption about the nature of the real is *à priori* more probable than that of the scientific realist. Is it true that there are no other considerations that ought to be noticed in trying to find the relative *à priori* probabilities of the rival assumptions about the real beside those mentioned in the phenomenalistic argument? It is clear that in considering the *à priori* probabilities we ought to take into account *all* relevant knowledge and belief as well as that about the comparative success with which the two assumptions account for our perceptions. Now, as we have constantly pointed out, people invariably begin with the conviction that what they perceive is real. It is only by arguments that they can be led out of this position, and when they pass out of it to the belief that what they perceive is appearances, the perception of which is caused by a reality, they still do not maintain a perfectly open mind as to the nature of that reality. It is quite true that purely on the grounds of the law of causation there is no reason why a cause should resemble its effect; some causes do so and others do not. But people do not decide here solely on the grounds of the law of causality. They suppose that in this particular case the cause does resemble—not indeed the effect, which is a psychical event, and is therefore more like the cause on the phenomenalistic theory—but the object of the perceptions that are the effects. If we started with perfectly open minds from the position that our perceptions as psychical events have causes which exist but cannot be perceived, then the phenomenalistic argument that it is *à priori* more probable that those real causes should be of the same general nature as perceptions than that they should be of the same general nature as the objects of perceptions would, as we have seen, have a certain though not a great weight. But when we are considering the *à priori* probabilities in this subject we must take into account, not merely the law of causality and the amount of knowledge and ignorance that it allows us, but also any other relevant judgments about probability that may be made on this point. Now it seems to me clear that people do think that it is more likely that the real shall be like the objects of their perceptions than like their perceptions as psychical events. This is not a belief, as we have seen and shall see, to which the standard arguments for subjective idealism[1] can bring any valid objection. Of course there are many beliefs which are very generally held, and the opposite of which it is very difficult to believe, which are as a matter of fact false. But I am not suggesting that it is more probable *à priori* that the real world should be like the objects of our perception than like the perceptions themselves, *because* most or all people think so. I merely wish to suggest that when a man recognises that there is no reason why the real world should not resemble in character the objects of his perceptions he will undoubtedly hold that it is more probable that it does than that it does not, and that this belief must be taken into account by every man who has it when he considers the relative probability *à priori* of phenomenalism and scientific realism. Our beliefs may be wrong, but so long as we have them and no valid argument can be found against them, it is our duty to judge in accordance with them, and not to pretend that they are non-existent.

I think it is fair then to conclude that the phenomenalistic argument, when superposed on the belief that our perceptions have causes, does not succeed in making it *à priori* more probable that the nature of those realities should be like that of our perceptions rather than that it should be like that of the objects of our perceptions. We will pass then to the relative final probabilities of the two theories, i.e. the probabilities that they gain by the agreement of the perceptions that they predict with those which we actually have. This takes us to the question which on p. 168 I called (*b*). If two hypotheses be equally probable *à priori* and explain known facts, their relative final probabilities are dependent on how well they explain the facts. By explaining the facts is meant making that probable which is actually found in most cases to be true. In the present reference there are two points to be noted: (1) Whether it is necessary at all to go outside the perceptions that we now have and can remember; and (2) whether, if we do so, laws entirely in terms of perceptions will explain better than laws in terms of realities whose general nature is like that of the objects of our perceptions.

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[1] It should be noted, however, that these do not exhaust the arguments on which Idealism has been based. The arguments for *esse=percipere* do not depend, as Mr Moore seemed to hold in his *Refutation of Idealism*, on the doctrine that *esse = percipi*. He forgot such arguments as the Hegelian Dialectic and Lotze's doctrine that substances must be selves. I do not think that these arguments prove idealism, but this is not the place to discuss them.

It is quite clear that if we keep entirely to what we can remember we shall be able to find but few causal laws among our perceptions. We must therefore give up all hope of being able to predict what perceptions we are likely to have in given circumstances or else assume something beside those perceptions that we can at a given moment reflect upon directly. The assumption that we make will have its initial probability increased in proportion to the success which it has in accounting for what is found to be true. The question that remains therefore is what assumption does this best.

Phenomenalism has a plausible argument to prove that the assumption of real causes of our perceptions like their objects in general character cannot explain better than the assumption of perceptions, because the hypotheses are really equivalent. This argument runs as follows: Suppose you do assume that your perceptions are *caused* by realities that obey certain laws among themselves. The only evidence for the particular kinds of realities and for the laws that they obey are the regularities that can be observed among your perceptions. Surely, then, it would be just as well to say that these regularities justify causal laws among perceptions of the form 'the occurrence of the perceptions, p, q, r at t makes that of s, t, u at $t + T$ probable,' as to say that the first set were caused by X, that X makes Y probable and that Y causes the second set. The argument then is that you do not directly need to assume anything fresh. You say that the observed regularities among perceptions are evidence for certain causal laws among them. Then no doubt you will find that these sometimes inexplicably break down unless you assume that you had perceptions on which you cannot now reflect, i.e. forgotten perceptions. The first point to notice about this argument is that, as we have already seen, the amount of regularity among the perceptions that can be remembered at any given time is very small indeed. As a matter of fact, we all assume on no grounds at all that what we perceive is more or less the same as what is real, and *when* this assumption is made as it is, it enables us to discover and make probable very many regularities which without it would not have been noticed or judged probable. The suggested laws of the form mentioned above connecting the occurrence of perceptions with each other could certainly never have been discovered if it had not been the case that by nature we make the assumption that the real world resembles the objects that we perceive. The phenomenalist then will have to regard our incorrigible tendency to make this assumption as a '*felix culpa*,' like that which led to the Redemption by way of the Fall. Still I do not think that we have any right to hold that because we should not have discovered certain propositions if we had not made certain assumptions, therefore the assumptions must be held to be more probable than the propositions which would not have been discovered without them. The essence of the phenomenalist argument is that, in as far as the assumption of the real world with its laws could ever give a verifiable result, it could be replaced by hypothetical propositions about the occurrence of perceptions, and that as far as it could not give a verifiable result it was useless.

I think, however, that the position of the phenomenalist is open to criticism. It unquestionably follows from the laws of probability that if a proposition or set of propositions p strengthen the probability of a proposition q, then if q be found to be true the probability of p is strengthened. Now it is quite clear that the assumption of a real world with certain laws is a set of propositions that strengthen the probability of what is actually found to happen. And it is also clear that it is a different assumption from that of laws connecting perceptions, even though it always has to be verified, if at all, by noting the occurrence of certain perceptions under certain circumstances which must also appear as perceptions. Hence it is quite certain that this particular assumption, like any other that makes probable what is as a matter of fact true, has its probability increased. Of course the phenomenalist would be perfectly right if he said that the discoverable order in our perceptions does not by itself prove the existence of a regular world of reals that cause them. For if any regular system led us to another regular system as probably existing, we should have no right to stop at the first external world. Its regularity would make it very probable that there was another that caused it, and so on. The fact is that the strengthening of probability takes place multiplicatively, and, if the *à priori* probability of the assumption be small, the final probability of the existence of such an assumed world will be negligible. Hence you cannot prove to a man who thinks it very unlikely that anything exists except perceptions, that it is probable that a real world like the objects of perceptions exists on the ground that it explains so well what actually takes place.

But, granted that no one really is in the position that he thinks it positively improbable that there should be a real world like the objects of his perceptions, except through listening to erroneous arguments, we must compare the strengthening of the probability of such an external world with the strengthening of the probability of laws connecting our perceptions, which are equivalent to the former in all cases capable of verification. Let us take some simple scientific law and consider it—say the proposition that 'heat causes metals to expand.' The ordinary theory would be that the metals, the temperature, and the length exist whether we perceive them or not; that, under suitable circumstances, we can perceive them; and that, when we do so, we are actually able to perceive that connexion which we believe to hold between change of temperature and change of length, whether we perceive it or not. The phenomenalist position is that it is absurd to assume any more than the law about perceptions, that whenever we have a perception of a metal with a certain degree of temperature we can perceive a certain length, and that this will increase as the felt temperature increases. This, the phenomenalist would say, is all that ever could be verified in the realist's law, and therefore all in his assumption that any verification can render probable. Now if all laws were as simple as this the phenomenalist might be right; but this is not the case. The actual position that we have to face is the following one: In general it is not a case of finding regular conjunctions of perceptions and arguing from this that there is a causal law of the form that the occurrence of p_1 strengthens the probability of the occurrence of p_2 and makes it practically certain. There are some such regularities among our perceptions, and if we confined ourselves to them, I think it would be true to say that such hypothetical propositions about perceptions were better justified than the assumption of real connexions of events that take place alike when we do and when we do not perceive them. But the propositions at which we arrive as probable when we assume a real world like the objects of our perceptions, are not propositions at which we could have arrived from any actual recurrent series of connected perceptions, for the excellent reason that such a recurrent series will not in general have taken place. Thus we must not merely say that the assumption of real causes of our perceptions obeying certain laws enables us to discover regularities in our perceptions that we should otherwise miss. What we must say is that the assumption actually renders probable propositions about the connexion of our perceptions that would not be rendered probable by our actual perceptions alone, and which are verified. Propositions of the form 'if p occurs it is practically certain that q will occur, where p and q are perceptions,' can only be rendered probable by our perceptions when p and q actually do constantly occur together. But in general both p and q scarcely ever occur at all, and therefore it is true to say that our perceptions alone do not render such propositions probable. But this just means that unless the assumption is probably true these propositions are not probable. Now the less probable a proposition is antecedently, i.e. without the assumption of a given proposition, the more probable does that assumption become if it makes the antecedently improbable proposition soluble, and the latter is as a matter of fact found to be true.

Let us take an example from the wave-theory of light. It can be shown to follow from this that there will be a bright spot in the middle of the shadow cast by a small circular object like a coin. Now the phenomenalist would say: 'Why suppose that this verifies the wave-theory in as far as that involves what cannot be perceived? Why not say at once that it makes probable the law that I shall always perceive a bright spot in the middle when I perceive a circular shadow. This is the only part of the law that can be verified. True I should never have thought of looking for a bright spot there if I had not made the assumption of the wave theory, but this merely shows that the assumption was a fortunate aid to the discovery of a hypothetical proposition connecting my perceptions.' In accordance with what we have said, the following is the right answer to this contention. Certainly it is perfectly true that what has been discovered involves a hypothetical law about perceptions. But if you keep to perceptions the law was not probable unless there was an actual coexistence in all or most cases of the shadow and the bright spot. Now, as a matter of fact, on your theory there was not such a coexistence. It is useless for you to tell us that there was a bright spot, but that you did not notice it, because you did not expect it to be there. If phenomenalism be correct it was not there when you did not notice it. Hence unless the assumption of the wave theory be true, or at least probable, your law about perceptions and their connexion does not merely remain undiscovered, it actually has practically no probability. On the other hand, when you have made the assumption of the wave-theory, and learnt from it that such a spot ought to be found, the fact that you find it and that antecedently to the assumption there was practically no probability of its being there strengthens the probability of the assumption enormously. In fact, the whole criticism of the phenomenalistic argument under discussion might be put as follows: Propositions asserting constant conjunctions hypothetically can only be rendered probable either by the actual observable recurrence of both terms in conjunction or by the fact that they can be inferred from probable hypotheses. In most of the hypothetical laws about the conjunctions of perceptions a constant recurrence of both terms in conjunction is not as a matter of fact experienced. It follows that such laws are not probable unless they follow from some probable hypothesis. But if such an hypothesis can be found, and the conjunction can be verified, even in a single case the very fact that, apart from the hypothesis, it had no particular probability, makes the probability of the hypothesis stronger.

We cannot therefore accept the phenomenalistic position that we may just as well assume hypothetical laws about our perceptions, as causes of them obeying among themselves the laws laid down by physics. The laws of physics will indeed in general be able to be stated in terms of connexions between possible perceptions; but the laws so stated will have little or no probability apart from the truth or probability of the assumption about the real world and its laws. And the mere statement of the laws of physics in terms of possible perceptions would not be without difficulty. Atoms, molecules, and ether-waves could not be put directly into phenomenalistic laws. For they are supposed to be of such a nature that they could not possibly be perceived. Hence a phenomenalistic law about them would take the form: If I had the perception *p* which, *ex-hypothesi*, I cannot have, I should always have also the perception *q* which, *ex-hypothesi*, I cannot have. If, then, we are to be able to carry science to any pitch it is essential that we should be able to allow the existence of real causes of our perceptions as at least possible, and when this is done the most probable laws for them to obey among themselves are those which science finds it necessary to assume in order to account for what is perceived. Now these laws are not in the least like those which perceptions obey among themselves, although they are of course connected with the latter. They are in fact laws about the kind of changes that we can observe in the object of a single continuous perception; and the only common characteristics of the objects of our perceptions and the perceptions themselves is that both have temporal relations and can enter into causal laws. Hence, until anyone can make up a theory in terms of laws like those that hold between perceptions which will explain our perceptions better than the theory of science, we shall be justified in holding that if there be a real world at all it probably resembles the objects of our perceptions. But this is subject to the difficulties which have to be dealt with in the next chapter, due to the fact that it has been held that the causal theory of knowledge, which begins by assuming a real world like the objects of our perceptions, ends by overthrowing all knowledge of the real. To the discussion of that theory we now turn.

CHAPTER IV

THE CAUSAL THEORY OF PERCEPTION

At every step in the preceding chapters we have been brought against the causal theory of perception. We saw too that it tended to land common-sense in conclusions that it did not want. Common-sense started with naïf realism and only left it when and inasmuch as it was forced to do so. But it was forced away from it at certain points by arguments independent of the causal theory of perception, and, whenever this happened, it introduced the causal view. For no sooner had common-sense distinguished between appearance and reality than it was forced to give some account of the relations between them in those objects which it *perceived* as being all of a piece, and, but for difficulties, would have continued to regard as all equally real. Unhesitatingly and without reflexion it rushed to the causal theory. The reality caused the perception of the appearances, and from the appearances you could get information about the reality. But the theory, whether true or not, was a bad ally for common-sense realism. It was not content to act as a humble account of the connexion of those two aspects which common-sense had been forced to separate. It could not leave common-sense with that amount of naïf realism that it wanted and that other arguments allowed to it. For the causal view itself becomes the strongest argument against naïf realism, and, when pursued, seems to leave it no foothold in the world. Educated common-sense followed it in its rejection of secondary qualities, but refused to give up primaries, and so matters stand to-day with science — except when it becomes phenomenalistic. But it was impossible for the philosopher to stop there; he was hurried on to the Thing-in-itself, and, in its mysterious presence, according to temperament he worshipped with Mr Spencer, constructed moral fairy tales with Kant, or began to ask, as we propose to do, whether his leader—the causal theory—which he had so innocently followed might not be a will o' the wisp.

We propose to discuss in the present chapter the arguments against naïf realism that depend on causation. We shall have to enquire where precisely they will let us rest and what precisely is meant by the view which is certainly held vaguely by educated common-sense that our perceptions have causes and that some relation is to be found between the nature of these causes and the reality of the objects perceived. The essential argument that we have to examine is that from the relativity of objects of perception to an organ; but we shall find that the causal theory once introduced exerts an influence on all the other arguments which we have previously discussed without assuming it.

It seems to be held that relativity to an organ is fatal to the reality of sense qualities. The statement to begin with is vague, but at the very outset we must carefully distinguish it from the position that I discussed in the last chapter which held that, since one of the qualities which reflexion shows to belong to every object that we perceive is that of standing in the relation of being perceived, there can be no ground for supposing that any object that can be perceived ever exists out of that relation. The argument here to be discussed introduces indeed a relation; but it is a relation to the body and not to the mind. Let me quote Bradley as an exponent of the argument under discussion. 'A thing is coloured, but, except to some eye, it seems not coloured at all. And the eye... relation to which appears somehow to make the quality—does that itself possess colour? Clearly not unless there be another eye that sees it. Nothing therefore is really coloured; colour seems only to belong to what is itself colourless. And the same result holds with heat and cold[1].'

There is clearly an analogy between this argument and the one for pure phenomenalism. It says: No doubt when you perceive red you do not at the same time perceive a relation to your eye. Still, in considering whether red be real or only an appearance, you ought clearly to take into account all that you can possibly find out about it. Now one of the things that you can find out about it is that it is never perceived except when it is also possible to find that it stands in certain relations to your eye. What right then have you to believe that it can exist out of those relations? The argument is then confused with the formally similar one for phenomenalism, because the relation mentioned in it is confused with that of being perceived, which plays the same part in that argument. It is, however, easy to see that the two relations cannot be identical, since relativity ~~to an organ cannot~~ possibly be the ~~same as relativity~~ to a percipient mind.

causal

existential

Our first duty then will be to clear up the relations between the two arguments. Call the present one *b* and the phenomenalistic one *a*. In the first place *b* is no ground by itself for believing the phenomenalistic conclusion. The fact that whatever I perceive has a certain relation to an organ of perception cannot possibly be by itself any reason for supposing that it does not exist when it is not perceived. For the relation to the organ, whatever it may be, *is* not the relation of being perceived, since that is a relation to the mind and not to the body. Hence, although *b* might prove, e.g. that there is no reason to suppose that red ever exists out of some relation to the eye, yet, as this relation cannot itself be that of being perceived, there can be no reason supplied by *b* alone that red does not exist in this relation *R* to the eye even when it is not perceived. It is clear that to prove the phenomenalistic conclusion we need something more than *b*, viz. a premise to the effect that the relation *R* to the organ of sense, whatever it may be, implies also the relation of being perceived. Without this we merely know that we never do perceive *X* without being able to show that it is also related to some organ; and this leaves two alternatives open to us beside phenomenalism, viz. (i) that the object continues to exist in the same relation to our organs even when we cease to perceive it, or (ii) that, whilst it cannot be perceived when it ceases to stand in this relation to the organ, yet it does not cease to exist when it ceases to stand in this relation. And, as before, the sole reason so far for choosing phenomenalism rather than one of these alternatives is the old argument *a* which we have already discussed.

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[1] *Appearance and Reality*, Chap. I. p. 12.

So far I have taken the relativity argument quite generally, and have shown that, unless the relation *R* to an organ which it claims to discover can be proved to imply the relation P of being perceived, it has no bearing on phenomenalism whatever. We will now consider R a little more closely and see if there be any reason to suppose that it implies P. What is the nature of R? It is clear that at this stage we must not tie ourselves down to the causal theory which common-sense commonly assumes as the connecting link between appearance and reality when it thinks it has discovered a distinction between the two. For the perceived relation is supposed to connect our organs of perception with what we perceive and the argument from it is supposed to show that what we perceive is always an appearance. On the other hand the causal relation is supposed to connect our perception of a given object taken as a mental event with the supposed real cause of that perception which is not itself the object of it. Another point to notice about R is that it must be a relation of whose existence and nature we must be able to learn, and we must therefore be careful that we do not, by assuming that the presence of R makes the objects of our perception appearances, assume that R itself is at once an object of a perception and a reality.

It is tolerably easy to see by considering particular cases what sort of a relation R must be. We shall see that it is not supposed to be a perceptible relation, and that is as it should be. Let us consider sight for example. I see a coloured object. I shut my eye or am deprived of it by accident and I no longer see such an object. So too if I turn my eye away and replace it by my ear. Again my eye is a very complicated organ in which various distinct parts can be found, and, by lesions of the eye and the comparison of it with other optical instruments I can discover approximately what *rôle* each part plays in the act of visio

We can find reason for supposing that the rods and cones are connected with colour vision, and convergence and accommodation with the perception of distance. The relation R then splits up into several distinct ones in the case of sight. (*a*) Without eyes or with them shut I can see nothing. (*b*) Some at least of the distinguishable characteristics in the object perceived cannot be perceived in the absence of certain appropriate structures in the eye. (*c*) With open eyes possessing all the appropriate parts in working order what I perceive depends on the position of my body. To put the matter more generally we must say: (*a*) Without special organs you cannot perceive the special qualities like sound, colour, extension, etc. (*b*) With such organs there is reason to believe that some of the special characteristics of these qualities are so connected with the detailed structure of the organ that in its absence they are imperceptible. And (*c*) with the appropriate organs in full working order what you perceive will still be a part conditioned by the position and past history of your body. It is only in the case of sight and hearing that much progress has been made in correlating distinctions that can be perceived in those qualities that cannot be perceived without the organ with the structure of that organ.

We can now at least be clear about the various relations with which we have to deal. There is firstly the [illegible] relation P of an object to the mind when that object is perceived. And then there is the
caus relation R between the object and the body which has been found to split up into the three relations r_1, r_2, r_3. The relation r_1 is that without the appropriate organ we cannot perceive qualities of a given sort. To this the main exception would be

perceptions in dreams. r_2 is a relation involving the particular characteristics of qualities (e.g. the particular colour or sound). It is such that without certain distinctions in the structure of the appropriate organ we cannot distinguish these particular characteristics. The relation r_3 is between the position and past history of our bodies and what we perceive. The question is whether the fact that whenever objects stand in the relation P to the mind they also have the relations r_1, r_2, r_3 to our bodies, is any reason for supposing that they cease to exist when they cease to stand in the relation P.

// With regard to the relation r_1, it seems to me tolerably obvious that it proves nothing. The fact that without eyes no one would perceive colours can be no reason for supposing that, granted that people have eyes and perceive colours, those colours cease to exist when they are not perceived. The possession of this structure might be the only way in which a mind that is connected with a body could perceive colours which would be there whether it perceived them or not. It is generally, and I think justly, held that there might well be sensible qualities that we do not perceive just because we have not the requisite organs. Electrical and magnetic qualities might perhaps be offered as examples. And there is a singular inconsistency in the position of a man who holds that the fact that without eyes we cannot perceive colours proves that colours are only appearances, and *also* holds that the fact that we dream about colours proves the same conclusion. When we dream about colours the objects of our dream perceptions are as coloured as those of waking life; the only difference is that we perceive them with our eyes shut. It is therefore an undue attempt to make the best of both worlds which proves that colours are mere appearances both because we can and because we cannot perceive them without using our eyes.

But it will be said that it is not the fact that we must have appropriate organs in order to perceive definite sense-qualities that proves that the latter are mere appearances, but rather the necessity of the relation r_2. It will be said that this is a mere rhetorical flourish preparatory to the really fatal argument that when you have got the organs and thus are able to perceive the general qualities the perception of the particular characteristics depends on the detailed structure of the organ. But, taken by itself, this argument seems to be precisely in the same position as the argument from r_1. It is impossible to tell *à priori* whether the detailed structure of the organ is the condition by which the mind perceives real distinctions or whether it merely conditions the production of the perception of an apparent object with these apparent distinctions in it. Either explanation will account for the facts.

The argument is often reinforced by considering how different the world must look to birds and insects with their eyes so widely different in structure from ours. It will be said: We have every reason to think that, even if there be something in common between what is perceived by all beings that have eyes, yet shapes, distances, and no doubt colours, will depend on the different structure of their organs. Think then how different must be the detail of what we perceive with our couple of movable adjustable lenses from what an insect perceives with an immovable eye consisting of hundreds of lenses set in different directions. What right have you to suppose that the object of *your* perception alone continues to exist unchanged when you cease to perceive it? There seems to be something in this argument which makes it worth while for us to consider precisely what it proves or renders probable, rather than hastily to reject all sense-qualities from the real, as is often done.

As stated the argument is open to criticism. We are told in one breath that the internal structure of the insect's eye obviously differs very much from our own and also that for this reason it is presumptuous to suppose that the spatial characteristics that *our* eyes discover in their objects of vision hold when we cease to perceive. Well, if that be so, what reason have we for supposing that the insect's eye really does differ from our own? Our only reason is that it looks to differ; but then it is presumptuous, we are warned, to suppose that the distinctions that we perceive are those which exist when we cease to perceive them. Hence it is presumptuous to believe that the insect's eye really does differ from ours; but it was the supposed difference in the first instance that made it presumptuous to trust our own sight.

The argument can, however, be freed from this defect. What it wants to be able to hold is that, whilst there is no particular reason to suppose that what I perceive is real, there is reason to believe that the perceived differences of structure between my eye and an animal's are marks of a real difference in structure. Although differences in the objects perceived may not themselves be real characteristics yet they may well be marks of real characteristics and this would be enough for the present purpose. When I perceive differences in the structure of two eyes and conclude that they must involve some difference in the characteristics of the objects perceived by means of them, I am not forced to believe that in order for

Why [illegible] ?

here to be a real difference between the eyes at all must consist in the reality of the perceived differnces. It is enough that these perceived differences hall be marks of real ones. In that case I can onclude that the real structure of the insect's eye iffers from that of my own and therefore that in ll probability what he perceives is different from hat I do. The premise on which this conclusion based can be shown to be probable. When we ave no information whatever about the nature of correspondence' between appearance and reality it ill be improbable that two objects that look different ill have the same reality 'corresponding' to them. or the corresponding realities can be identical in ly one way whilst they can differ in an indefinite umber. Hence, when nothing more is known except at the objects of one's perceptions are appearances, e alternative that what corresponds to two different pearances is entirely the same is only one alterative out of an infinite number. The real structure f the insect's eye might differ from that of our own an indefinite number of ways; it could only be lentical with it in one. Now the fact that the erceived objects differ certainly does not add to the lready small probability that the corresponding realiies are identical. Hence it seems fair to conclude hat the perceived difference between my eye and hat of an insect is a mark of a real difference in tructure and therefore that it is probable that the nsect does not perceive precisely the same object of erception as I do.

Granted then that our experience does give us ome good ground for supposing that insects perceive lifferent objects from what we do in respect of detail, an the rest of the argument be maintained[illegible] The rest of the argument was that under these circumstances it was very presumptuous to suppose that what we perceive is real rather than the different object which the insect perceives. This is supposed to force us to the alternative that neither of the two objects ~~is real. Clearly some further~~ argument must be needed to prove this, since anything that has yet been offered leaves open the alternative that both are real even when we reject as unreasonable the belief that one is superior to the other in reality. Now the only ground for denying that both what the insect sees and what I see are real is the old one that we mentioned in the second chapter that we otherwise multiply real entities to a terrible extent. For the position is quite comparable with that of the innumerable different ellipses that we see as we move about when we say that what 'we really perceive' is a circle. The alternative then was open to us to hold that all these were equally real, but that each could only be seen from one position and that none of them could be felt. Here again it *might* be that both what we perceive and what the insect perceives are equally real, but that only minds furnished with eyes like ours can perceive the reality that we do, and only those furnished with eyes like insects' can perceive the reality that they do. And once more I must insist that until you assume that all these perceptions of very similar objects have a common real cause there is nothing improbable in a world of reals, each of which can only be perceived by one creature from one place. Still, as we know, that is not the alternative that anyone adopts. We always assume that realities are dear and perceptions of appearances cheap and prefer to shape our theories of perception so as to lessen the former at the price of increasing the latter.

What then I wish particularly to point out is that once more the relativity argument has proved powerless by itself to show that the objects of our perceptions are appearances rather than that the structure of our organs is the necessary condition of our perceiving certain special qualities and characteristics of reality. In the argument that we have just discussed it has merely succeeded in leading us back to the argument I (*b*) of our first chapter and in providing some new special cases to which to apply it.

We have now seen how far the argument from relativity to an organ, in as far as it rests on a correlation between the observed structure of our organs and the characteristics of our objects of perception, is relevant to the question of realism. We have seen that all the facts are capable of two interpretations, viz. the Instrumental one which holds that our organs and their detailed structure are the instruments by which the mind perceives real things and their real qualities and characteristics; and the Causal one which holds that our organs and their internal structure are conditions of the perception by the mind of objects and distinctions in them, both of which for aught we can tell are mere appearances. So far the only means that has been offered for deciding between the two views is the old argument from complication of reals.

Our next step will be to discuss these two alternatives further and to consider the extraordinary way in which science and common-sense divide up given masses of perceptions as between these two views. We may then perhaps hope to discover some principle of separation which we can criticise. Where correlation can be found between organ and object perceived common-sense will sometimes hold that the bodily conditions are what enable the mind to perceive certain characteristics that continue to exist whether they be perceived or not, and in other cases it holds that the detail in the organs is the actual cause of the detail in the object perceived and that the distinctions in the object cease to exist when we cease to perceive them. We will cite some examples and then try to discover some principle of distinction and to criticise it. (i) The fact that we need definite organs to perceive definite qualities and that the perception of detail in the object perceived is found to depend in some measure on the structure of the organ is not generally thought by common-sense to prove that the qualities and the details perceived are mere appearance. It is rather held that the organ may be regarded as an instrument like a hammer or a chisel which the mind uses to perceive qualities and distinctions which are really present whether they are perceived or not. As we saw we only begin to doubt this when we consider that in all probability other creatures must perceive things as rather different from what we do, and ~~that~~ it is difficult to give any prerogative to our organs as telling either the whole truth or nothing but the truth about reality, and equally repugnant to believe that what each of us perceives is real but imperceptible to the other. (ii) The fact that as we move our bodies about what we perceive is seen to change shape steadily is held to prove that we do not perceive the real; for it is held that in general these changes do not take place in the real that 'corresponds to' our objects of perception. Similarly the double objects that we perceive when we push our eyes aside are not supposed to be real. Thus this sort of dependence on the positions and states of our organs is supposed to prove that what we perceive is appearances and that the change in our organs causes them to alter, and not that the real alters and the changes in our organs are the means by which we perceive this real alteration. (iii) The use of instruments on the other hand seems to be held to be comparable with that of the detailed structure of our organs and to be the means by which we perceive real distinctions which we could not perceive without them. Yet (iv) not all instruments are supposed to do this. Some are believed to distort the real, i.e. to show us characteristics that are not there except as perceived. And the taking of certain drugs or excess of alcohol is supposed to have the same effect; i.e. not to reveal new details in reality but to make us perceive more appearances.

Here we have a wide and difficult subject for discussion and criticism. We must first of all distinguish the Instrumental View from the rest. The Instrumental View refers to the relatively permanent structure of our bodily organs and holds that they are the means whereby our minds get at real distinctions. So far is this view carried that, unless the proper instrument be employed, what is perceived is not real. This is one reason why it is held to be obvious that what is perceived in dreams is unreal. 'You had your eyes shut when you saw this,' which would be the usual criticism of a dream perception would reduce to 'You were not using the instruments which everyone knows to be essential if you wish to see the real.' Of course there are other arguments about dreams which I have also discussed; but this would certainly be regarded as one important argument. The present view also holds that there are some additional conditions that the mind can usefully employ to help the bodily instrument, viz. certain arrangements of matter like microscopes. These, although not permanent conditions like the eyes and their structure, are yet held to be means of getting information about the real that could not be discovered without their help. Again, bodily instruments, like other instruments, are held to need adjustment and arrangement in proper positions in order for them to enable the mind to perceive the real. And this raises the difficulty of how we can tell when they really are in adjustment. And we also have to try and understand how it is that some instruments and some bodily motions can be known to help the mind to perceive the real whilst others can be known to make it perceive appearance.

I think then that we can discuss the whole problem under the following heads: (i) Can we maintain that the bodily organs are instruments by which the mind can learn about the real in perception, and that the dependence of the perception of certain characteristics in the objects perceived on the detailed structure of the organ only shows that this detail is necessary for the perception of real characteristics and not that it is the cause of the perception of merely apparent characteristics? (ii) Can we give a reasonable account of what we mean by these bodily organs being in proper adjustment which will enable us to hold that under certain conditions the objects perceived by them are real, but under others—such as the pushing aside of the eye or the change of shapes with motion—they are not real? And (iii) Can we give a reasonable account of instruments of precision which will justify the view e.g. that an object seen through a microscope gives us new and true information about reality, whilst the universal yellowness seen after taking santonin does not?

(i) *The Instrumental Theory.* First of all we must try to understand precisely what is meant by an instrumental theory of perception. We will begin by considering the analogy with tools, and see how far it holds. Let us consider a typewriter. This is a material mechanism consisting of parts so connected that by using it properly the hand can print letters on paper in any order that the mind chooses. The letters that are printable are correlated with differentiations of the typewriter itself. Now it is obvious that there is some analogy between an arrangement like this and an organ like the eye. The eye also is a differentiated structure; without it (if we set aside dreams) we can perceive neither visual extension, shape, distance, nor colours. With it there is strong reason to believe that some at least of the particular characteristics of these general qualities are correlated with certain differentiations of its structure. So far the analogy to the typewriter is pretty close. But now we must note some differences. (i) That effect of the typewriter's working for which we say it is the necessary instrument is produced in a third thing different both from that which employs the instrument and from the instrument itself—viz. the paper. Now, in vision with the eye, on the assumption that the eye is an instrument by which we come to perceive reality, we must have quite a different arrangement. In the case of the typewriter there was (*a*) the mind and body of the typist; (*b*) the machine with its internal structure, and (*c*) the blank paper. As a result of changes in *a* effects are produced in *b* which in turn produce effects in *c* which are correlated with the differentiations of *b*. In vision with the eye we seem to have (*a*) the mind, (*b*) the eye with its internal structure—(we ought of course to include the brain and nerves, but the point is unimportant for the present purpose)—(*c*) the real object with its real differentiations, and (*d*) the effect, which is presumably the perception by the mind of the object with its differentiations. But in the first case *d* was a change in *c* caused by *a* through the use of *b*. Now in the present case this is precisely what is not supposed to happen. If the act of perceiving *X* changes *X* then it is not *X* that we perceive. In fact the change is supposed to be produced not in *c* but in *a*, i.e. in the mind that uses the instrument. If follows that, even on the instrumental view, the effect is a change in the mind, so that the instrumental view has at once some affinity to the causal one.

The next point to notice is the peculiar nature of the effect. Knowledge and perception are of course unique events, and it is not to be expected that an example taken from material instruments used to affect the material world will furnish a complete analogy. Unfortunately there is not a general agreement as to the precise nature of the effect. The difficulty is that people are not agreed as to what they mean by a mind. But, waiving that difficulty, and supposing that we do know more or less what we mean by a mind even if we should be hard put to it to express our meaning in words, I suppose we could agree that the effect is what we call a perception and that a perception is something that can be analysed conceptually into a mind in the relation of perception to an object. It is an open question whether these terms can ever exist out of relations of that kind, but that of course is precisely the question that we have all along been discussing. All that can be said about the relation is that it must not alter the qualities of its referent otherwise than to give it the quality which it did not, if real, possess before, viz. that of being perceived, i.e. of being *relatum* to this relation. Some such view as this must I think be admitted. It must be noted that here it is hardly possible to call the real object a cause of the perception, except that it may be something in the object that causes a mind provided with the appropriate organs to enter into this relation to that particular object rather than to another at a given time. But this does not depend, I take it, on that particular character that it is perceived to have in distinction from other objects of perception; for *all* objects that are real and are perceived at any time must agree in possessing in them the causes necessary to set up the relation to the mind if those causes lie in the perceived objects at all. This is quite different from the case of the typewriter. There is not something already written on the paper, nor does something in the paper other than the writing set up a relation by means of the typewriter between the hand and the written paper.

It is the belief in error that forces us to desert a purely instrumental view such as I have sketched. We have some reason to believe that we do not always perceive what really exists; at any rate if we believe it we have to deal with a terribly complicated world. Now the difficulty on the present view of the perception of appearances is obvious. They are not perceptions of nothing, but have an object just as much as do those which are supposed to be perceptions of the real. And the objects in both cases are clearly very much alike. The important difference is supposed to be that appearances, unlike real objects of perception, cease to exist when they cease to be perceived. But if perception by means of an organ just means that through its assistance mind and object are brought into the relation of perception to each other, this seems to imply that both are existing beforehand ready to be brought into that relation. And this cannot be so with apparent objects. The only way then to explain the perception of appearances on this theory would be to suppose that they begin to exist and enter into the relation of being perceived at one and the same moment. Now this is clearly the edge of a slippery slope. For now it seems as if the mind and its organs were, at any rate, the part-causes not merely of the entering of an already existent reality into a definite relation to the mind, but also of the existence of objects with their differentiations. If then the mind + an organ + perhaps some third factor are capable of causing not only the entry of realities into the relation of being perceived but also the existence of objects in that relation—objects which cease to exist when the relation ceases to hold—what ground can we have for supposing that *any* of the objects that we perceive are realities rather than that they are appearances caused in the second way, which is now admitted to be possible? In fact can you begin to distinguish between appearances and realities without making it very probable that we perceive nothing but appearances?

Thus the instrumental theory comes to be replaced by a causal one. In the causal theory something X acts on the organ, the organ and the mind together produce a perception as a whole, i.e. something from which indeed an object can be analysed out, though there is no reason to think that it can exist out of that whole called a perception. Such an object is an appearance in our sense of the word. As soon as all perceived objects thus become appearances the analysis of the perception into mind + relation + object tends to be dropped and replaced by a whole called a state of mind in which the difference of object is attached as a kind of adjective. For, since all these objects are appearances, and cannot exist, as is believed, out of this relation to the mind, it is felt that the old analysis is otiose. This is of course harmless enough so long as it merely means that, whilst the distinction between perception and object perceived is clearly recognised, it is also recognised that it is only an analysis like that of a note into its pitch, intensity, and quality, and that the elements elicited do not exist apart. But in practice it is a most dangerous step because the undoubtedly important difference between object perceived and perception of object, which remains on any theory, is minimised until it is thought that because the latter is mental the former must obviously be so too.

But we have been running ahead of common-sense and must now return to it. The replacement of the instrumental theory by the causal one is very slow and tentative, and common-sense believes that it can distinguish ~~between~~ cases where the instrumental theory still holds and others where the causal one becomes necessary. Let us consider the steps that common-sense makes. (*a*) The instrumental theory is believed at first to be sufficient for the ordinary perceptions of everyday life, and only mirages and bent sticks and such objects are held to require the causal view. (*b*) Further investigation shows that what different people see at the same time and what the same person sees at different times from different places often closely resemble each other and yet have discoverable differences. The likenesses and differences seem easily explicable on the causal theory by the assumption of a common cause and slightly different conditions. Hence the causal theory begins to spread at the expense of the instrumental one. (*c*) Speculative persons now suggest that in all cases what we perceive are appearances but that in true perceptions the objects perceived are like the real non-mental non-organic causes of these perceptions, whilst in illusory perceptions they are not. (*d*) Finally it is asked: What possible reason can there be for supposing that the causes of our perceptions in any way resemble the effects? Thus we reach the thing-in-itself. Ordinary common-sense wavers between (*a*) and (*b*); orthodox science (apart from Mach's school) wavers between (*b*) and (*c*). (*d*) is held with varying degrees of consistency by Kant, Spencer, and Huxley; but efforts have always been made to get away from it and to offer some further determination of things-in-themselves, e.g. the Will in Schopenhauer and the noümenal self in Kant. Common-sense apparently never supposes that the causes of our perceptions are past states of ourselves or of other minds. This alternative, according to the way in which it is worked out, leads to Solipsism, Phenomenalism, or Idealism.

We must now consider the position that the organs of perception are the mind's instruments a little further, and show some other distinctions between them and instruments like typewriters which are used by the body and the mind. In all use of instruments the user is supposed to be a substance and active. We saw that we could make little use of the conception of activity and that there were difficulties about substance except for objects in space. This view then will assume that the mind is a substance and active. By its activity we can only mean, after our discussion on causality, that events in it are part-causes of an effect. The effect here is however a change in the mind, as we have seen. Hence we have to distinguish (*a*) the change in the mind that caused it to use the instrument and (*b*) the change that this use of the instrument causes in the mind that uses it. All ordinary instruments like typewriters are voluntarily employed, thus, in their employment there are on the instrumental view of the bodily organs two separate uses of instruments—(i) the mind uses the body (here by means of volition) and (ii) the body uses the typewriter. We have to consider the first of these. In the use of organs by the mind for perception the use may be voluntary or involuntary. There are a good many perceptions that we cannot help having. On the other hand it is clear that the use of our organs is also in part voluntary. The volition enters when we voluntarily place and adjust our organs and when we pay attention. It is only in such cases as these that the mind's use of organs of perception is strictly analogous to the use of a typewriter. We voluntarily do certain things with our bodies just as we do with typewriters because we have reason to believe that the result will be one which we wish and cannot reach without the use of such helps. But, whilst typewriters will not work of themselves, i.e. someone must wish to write in order for them to write anything, minds can and do have perceptions by means of their organs even when they do not wish to do so. When the reference to volition as an antecedent to the use of the instrument is cut out, a good deal of the analogy between our organs and instruments seems to vanish. We must now say that the same mind which can use bodily organs to produce changes in itself called perceptions may also have those changes produced in itself by the instruments without having previously willed to use them. Now we know that, in the case of perception, the perception that is finally produced is an effect in the mind; but what was essential to a purely instrumental view was that the mind + the organ should also condition the perception. On the view of perception as a relation between mind and object the mind could only be said to be a cause in the sense that it depends on its nature that the relation of perception can be set up between it and anything else. Now even when our having a certain perception can be said to depend on our past volitions the immediate effect of those volitions is merely to adjust our organs and put them into definite positions. After that what we perceive does not depend, so far as we can see, on any other volition. But, for complete analogy between the use of an instrument by the body and that of an organ by the mind, it would seem, as we saw, essential that the mind should be 'active' in using the instrument, and that would mean that all the time it was being used events in the mind were going on and were part-causes of the effect. We see however that there is no evidence that this is the case either with voluntary or involuntary perception. In the first place, for the present purpose the former reduces to the latter, because, as we saw, the whole effect of volition is to produce a preliminary adjustment of the organ after which everything proceeds involuntarily. And in this involuntary process we cannot discover that the cause of the perception is the comparatively permanent structure of the organ + changing states of the mind. As far as we can see, the mind only enters as a kind of permanent condition in the sense that it is the sort of thing which alone can be referent to the relation of perception. Thus the analogy between perception by means of an organ and the use of an instrument like a typewriter breaks down over the fact that the user in the first case is not, and in the second case is, continually 'active' while he uses the instrument. The view with which we seem to be left is that organ + mind are joint conditions with some other cause of the mind entering into the relation of perception to an object.

What however was the essential element in the instrumental view from the standpoint of the theory of knowledge? It was that the use of the organ gave us true knowledge about reality which we could not get without it. Now this possibility is not affected by the distinction that we have just noted between the way in which minds *use* organs and the way in which they use typewriters by means of bodies. Suppose that the organs + the mind are joint conditions, with some other cause, of the establishment of the relation of perception between the mind and some object; then the important question for the theory of knowledge is not directly what that third part of the cause may be, but whether as a matter of fact the term to which the three in conjunction cause the mind to have this relation is a real thing or an appearance. It would not be necessary in order that perception should give knowledge of the real that the third condition of the perception beside the organ and the mind should lie in the reality which is perceived in that particular perception. Yet common-sense has rarely accepted any other alternative. It has always wanted the cause to be like the object of the perception that it causes, and, in as far as it has failed to be so, it has held that object to be appearance.

We must take a less timid view however and put our question about the revised form of the instrumental theory as follows: Granted that the mind and the structure of our bodily organs are joint conditions which in combination with some other cause bring the mind into the relation of perceiving an object, can we discover (*a*) Whether that object is real, and, if so, when? and (*b*) What are the natures of the causes in particular cases?

Now common-sense in its own theory of knowledge has an answer to both these questions. In its less reflective form it says: (*a*) Nearly all the objects with which we enter into the relation of perception are real; but some ~~are appearances~~: (*b*) When our perceptions are perceptions of the real the cause of our perceiving just this thing at this moment lies in the reality which we perceive, ~~together with the joint~~ conditions that we have the appropriate organs and a mind. When we perceive an appearance the cause cannot be in the object of that perception, since that did not exist until the perception itself did so. It must therefore lie in other things which we do not perceive at the moment when we perceive the appearances. This seems to me to be the theory of knowledge of reflective common-sense, and, as it is carried over, in part at any rate, into natural science, we shall do well to examine it carefully.

This theory enables common-sense to give some account of the various ellipses that are seen from various positions and the real circle which it believes to be there. The real circle, it will say, has in itself the cause of our perception of it when combined with other relatively permanent conditions. The ellipses that are seen from other positions are appearances and, as such, it cannot be events in them that cause our perception of them. On the contrary it is still events in the real circle. These have remained the same but the conditions have changed slightly and therefore it is only to be expected from our general knowledge of causal laws that a slight change in the effect will be produced such as is observed. This then would be the way in which common-sense would fulfil its favourite maxim that 'realities are dear and appearances cheap' on the present view. We shall have later to consider whether this particular argument is justified even on its own premises.

At present however we will consider generally the view that we have attributed to common-sense. That view depends on the synthetic proposition that when real things are perceived, events in the real things are part-causes of the perceptions of them, the other conditions being (*a*) a mind, and (*b*) bodily organs properly constructed and adjusted. Is such a synthetic proposition true, and, if so, could we ever have been led to discover its truth?

The first point to note is that it contains two synthetic propositions. (*a*) Certain objects of perception have events in them which are causes of those objects being perceived; and (*b*) All ~~objects~~ that are real and are perceived have the perceptions of themselves caused by events in them. It leaves open the question whether there are real things that are never perceived, but common-sense would certainly hold that that was possible and probable. We have to enquire then whether these two synthetic propositions are true, or whether it is possible to get the sort of evidence necessary to prove or render them probable. They are certainly not self-evident, so that unless they can be rendered probable by some other propositions we have no particular right to believe them. We may therefore begin by asking whether there is any way of proving or rendering them probable.

Causal laws can only be elicited by observation either of the sequence of events which we finally decide to be cause and effect, or of other sequences which we analyse so that the causal law in question emerges as a product of analysis, or as a hypothetical law between what is not observed which what *is* observed renders probable. Now it is quite clear that we cannot observe first certain events in X and then note that later on we perceive X, for that would be to imply that we can observe parts of X before we observe any of it, which is absurd. Hence, if the causal law be one of succession it cannot be discovered by observing the succession directly. If the causation be contemporary it would be theoretically possible to observe directly that conjunction of events which might encourage a belief in a causal law. For we might observe that there was always a definite process going on in all objects that we perceive so long as we perceive them. But there would be very little comfort to be got out of this. In the first place we do not observe anything of the kind. And further, if we did, we certainly could not base a law of simultaneous causality on this alone; we should also want to know whether the same process might not be going on in the things when we did not observe them. And we clearly could not decide this point by observation, since we cannot perceive what things are doing when we do not perceive them.

It is perhaps worth while to note that if we are dealing with successive causation the converse of (*b*) will follow from (*a*). If there be any objects of perception, the perception of which is caused by events in those objects that happened before the perception took place, then it is clear that such objects must be real. For they must have existed before the perception of them took place in order that they should have states before that event, and if they were appearances this would be impossible. If the causality be simultaneous this will still follow. For the event in the object will then take place at the same moment as the perception, if it be the cause of the latter. But an event cannot be said to take place in anything unless that thing has existed for a finite time[1], and therefore if an event in an object be a cause even by simultaneous causality of the perception of that object the object must have existed for a finite time before it was perceived, and therefore be real. The same will hold if we allow the cause to follow the effect, though common-sense does not generally admit this possibility. For if an event in *X* be a cause of a previous perception of *X*, then *X* must exist after the perception of *X*, and therefore cannot be a mere appearance. If we allow an interval between cause and effect we must be prepared to admit that a mind may perceive an object either before it has begun or after it has ceased to exist. There is no particular objection to this. Common-sense would indeed reject the former alternative, but it holds that the latter often occurs, and, as far as I can see, there is no difference in principle involved.

After this digression we can return to the question of how it is supposed that we actually learn about the propositions (*a*) and (*b*). It will now be admitted that we cannot hope to discover by direct observation the causal laws whether simultaneous or successive that connect events in what we perceive with our perception thereof. The only possible method then must be the hypothetical one. Now we are also told by science that the cause of our perceptions of objects is imperceptible events in imperceptible parts of those objects if real. Science would certainly hold that the perceptions of dreams are appearances, and as we saw would probably give as one reason for this opinion that they are not perceived by means of the appropriate organs. But, if pressed as to why this should matter, I think it would almost certainly reply that as the organs are not used we must assume that dreams are due to central excitations, which just means that they are caused by our own bodies. And, if it be asked why perceptions completely caused by states of our own bodies should not be perceptions of real objects, I think there can be no doubt that science would answer that it is because the object of our dream perception is not a part of that body whose changes cause the perception. Thus there seems to be no doubt that science does in some way accept the common-sense view that the perceptions of real objects must be caused by events in those objects, although it cannot be quite so naïf about it as is common-sense.

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[1] For any change involves two different states at different moments, and all different moments are at finite durations apart.

It will be best then to formulate the theory under discussion in terms of the beliefs of science since we see that they only differ from those of common-sense by their greater elaboration and more consistent application of a common principle. We must then (1) formulate the position which natural science takes up on this matter, and (2) see whether it is a subject to which the hypothetical method really applies.

(1) We know roughly that science believes that extension, figure, mass, and motion are real, and that it believes itself able to prove, or at any rate to render very probable, that the causes of the perceptions, both of other qualities which it believes to be appearances and of those qualities which it believes to be real, are the motions of small extended masses with electric charges. It is true that so far, without introducing

causation, we have seen no reason whatever for ascribing a superior reality to the so-called primary rather than to the so-called secondary qualities, whilst, with such a theory, it remains a question whether either can be said to be real. However, we will set this difficulty aside for the present and consider the exact sense in which science believes that when we perceive a real object the cause of the perception of it is to be found in changes within it. In the first place, if chairs and tables really consist of little extended bodies in very rapid motion, can it be said that I ever perceive anything real, since I certainly do not perceive anything of this kind when I look at a chair or a table? If we throw away at once at the bidding of science all secondary qualities, this awkward question will still crop up with regard to primaries. I think that science would answer: You do not indeed perceive all that is real, nor indeed are all the particular shapes and sizes that you perceive real, but as far as concerns primary qualities, all that you perceive when your organs are properly adjusted either is real or is related according to known laws with real primary qualities. The chair that I see has a certain shape and size; subject to certain modifications, that remain to be discussed and are connected with the change of shape and size through change of relative position and adjustment of our organs, science would hold that we do perceive the real shape and size in a certain definable sense. That is to say, that, while we do not perceive the differentiation into electrons in rapid motion, yet the limits within which the electrons are moving about are practically those that are seen with the naked eye. Again, if the chair visibly moves, we do not indeed perceive the motions of individual electrons, but all the electrons are moving in the direction of the perceived motion of the chair as a whole, retaining the old geometrical outline. It is in this sense, then, I take it, that the primary qualities that we perceive under certain conditions with our organs are real.

We can now understand in what sense science means that when we perceive what is real the cause of our perception is events in the reality that is perceived. It means that the cause is events in the real which are not themselves perceptible, but are changes in imperceptible parts of that whose real shape, size, and motion as a whole we are caused to perceive by these events. This is an intelligible statement, but in it the view of common-sense is beginning to lose its simplicity. It will be noted that on the ordinary scientific view the instrumental and the causal positions are combined in the perception of an object. The events in that object are supposed to bring us really into perceptual relation with some at least of its primary qualities, and the eye is a necessary means to this end. But they *also* are supposed to cause states in our eyes which cause perceptions of colours, and the objects of these perceptions are mere appearances. If we had not properly constructed eyes we should not perceive either shapes or colours; but the second would not rob us of any knowledge about the real on this theory, whilst the first would. I think, however, that even among secondaries science would draw some distinction. It would almost certainly hold that the colours perceived on real tablecloths were in some way more reputable than those perceived in dreams, on the ground that the cause of the former was events in that of which there was a perception, whilst this is not so in the latter. I do not think, however, that anything can be made of such a distinction. Once a quality is believed to be an appearance it is surely impossible to smuggle back a kind of reality for it which is reflected from one of the other effects of the cause of our perception of it.

The final position of science then would seem to be that the perception of what is real must be caused by events in that real thing, but that those real events also cause the perception of appearances which may (as in the case of colours) or may not (as in that of sounds) be localised in that real thing by the percipient.

(2) As we now understand the position that science takes up as to the relation between the real and the causes of our perceptions of it, we can pass to the consideration of the method by which science claims to discover these imperceptible causes and hypothetical laws in the case of our perceptions both of appearances and of realities.

The method as we saw must be in the end hypothetical. Science has to establish two things: (*a*) That when we perceive the real there are differentiations in it that we do not perceive, and (*b*) The nature of the psychophysical laws connecting the imperceptible changes of these imperceptible realities with our perceptions. With regard to the laws, we ought to note that certain things can be discovered before we make any particular theory as to the nature of the more remote causes of our perceptions. These have indeed already been mentioned in part at any rate. It is independent of any particular theory that without eyes we cannot see colours and shapes and that therefore bodily organs are in general necessary for the perception of the distinct general qualities, whether they be real or only appearances. Again, the fact of perceiving similar things in dreams, and the results of lesions of the brain and nerves suggest that the last stage in the process would be a state of brain, and that when we do perceive by using organs, the organs have states which affect the brain, and the states of the brain are the last cause of the perception, both of appearances and of realities. Thus without any special theory about the nature of the complete causes of our perceptions, it looks as if we could distinguish three kinds of causal laws which successively operate in the causation of our perceptions. These would be (*a*) Physicophysiological laws which deal with the causation of the variable states in the relatively permanent structure of our organs; (*b*) Physiological laws connecting the states of our organs with the final states of our brain; and (*c*) Psychophysiological laws connecting the final states of our brain with the differentiations of the object the perception of which they cause. When we do not want to refer to this probable analysis of the total process we shall just refer to the whole process as Psychophysical.

What particularly interests us at present is the evidence for the physicophysiological laws, but it is hardly possible to consider these alone and apart from the total process. It is clear that our evidence must in the end rest on what we do perceive. Now all that we can be fairly sure about without hypothesis is the necessity of organs of a certain definite structure for us to be able to perceive certain general qualities at all. But of course we never do perceive qualities in general, but particular cases of them, and we perceive now one particular case and now another; now a green circle and now a red triangle. If we accept the instrumental view we are not forced to believe that there is a different bodily state for every different particularisation of general qualities that we perceive. The eye with its special structure might be essential to our perception of colour and shape and extension of any kind, but it might well be that when once we had the structure requisite for this we could perceive a red triangle and a green circle without there being any special state of body corresponding to red and green, to circle and triangle. Something would bring a mind capable of perceiving colour and shape now into perceptual relation with a red triangle and now to a green circle, and there would be an end of the matter. On the other hand, this is not the type of view that science tends to adopt as it advances. It expects to find not merely that a permanent structure of the organs is necessary for the perception of general qualities, but also that to every particular difference perceived there will be a different state in the organ. Thus it would expect to find that the eye really was in a different state when it perceived a red triangle from that in which it is when it perceives a red circle. It is clear that a great deal in this view must be as hypothetical as anything in the purely psychophysiological part of the process. No one can observe that there is a different state in the eye and finally in the brain for every different particularisation of every quality that is perceived. Still as time goes on some direct evidence is found. We can more or less correlate the accommodation and convergence of our eyes with the distance at which we perceive objects and prove that in their absence objects can only be perceived clearly at a single distance; and we can correlate, though with less certainty, the fibres of Corti with the hearing of notes. Still in the main the further developments of the physiology of the sense organs and their laws must be as hypothetical as electrons and the laws of light-transmission.

Now when we have got beyond what we can actually observe it is clear that from the same data, viz. our perceptions and their observed qualities, we have yet to substantiate two sorts of hypothetical entities at least and three sorts of hypothetical laws at least. Any perception will involve the hypothetical causes of the states of our organs and their laws; the hypothetical imperceptible states of our organs and of the brain and their laws; and the hypothetical laws connecting the states of our brains with the objects that they are supposed to cause us to perceive. We have, in fact, a large number of unknowns to determine from one set of knowns, viz. our perceptions and their sequences. It is clear, then, that there can be no perfectly determinate solution of the problem. You can make up various laws and various entities, and by suitably connecting them account for the facts. Still, if one theory can be set up whose assumptions are not intrinsically improbable, and which does account for the facts that can be observed much better than any other, and also intrinsically quite well, we may fairly assume that it is very probable.

In some measure this seems to be possible. For instance, we can tell by actual observation that certain permanent general qualities in what we perceive can only be perceived by people who have a certain perceptible organ and with a perceptible structure of a certain kind. And still keeping to the perceptible, we can carry such correlation a good deal further with great probability. Now, *if we can assume* that what we cannot perceive resembles in the main what we can and do, it is fair to hold that an hypothesis that carries such a correlation between the structure of our organs and the ability to perceive details still further, is intrinsically quite probable, and, if in company with other intrinsically probable hypotheses, it accounts well for what we do perceive, the total system will be probable, and will reflect an additional probability on its parts.

So much for physiological hypotheses and entities. Again, with regard to physicophysiological ones, subject to the same assumption of the probability that what is not perceived is like what is perceived, we may build up an initially probable hypothesis about the physical causes of the physiological changes that end in perceptions; and this, when combined with our physiological and psychophysiological hypotheses, themselves supposed intrinsically not improbable, will account so well for what we do perceive that it will furnish a probable theory and will reflect an additional probability on its elements. For instance, we might run very rapidly over the scientific arguments for the wave-theory of light to show the kind of argument with which we have to deal. It would go somewhat as follows: (i) It is a matter of direct observation that when we hear a sound we can generally find a vibrating body which we can see and feel to be vibrating. (ii) Another essential element is a medium between the body and the ear, and it can be shown that we only hear the sound after a definite interval from the beginning of the vibrations, which depends on the distance and the nature of the medium. (iii) There are a great many phenomena with regard to colours which can best be explained by supposing that the cause of our perception of them is also a vibratory disturbance; but the absence of a material medium is not found, as in the case of sound, to stop the experience altogether. (iv) There are numbers of phenomena which can be easily explained on the assumption that bodies are more differentiated than they appear to be, and only with great difficulty without that assumption. (v) On the assumption that little imperceptible bits of matter and little electric charges obey the same laws as larger perceptible ones, there is good reason to believe (*a*) that little charged particles exist in bodies under certain conditions, and (*b*) that when they vibrate they will emit periodic electric and magnetic disturbances that will travel with a calculable velocity, and will be executed at right angles to their direction of propagation. (vi) Experiments make it probable that light also travels with the velocity, and the effect of magnetic fields on polarised light make it very probable that the periodic disturbances mentioned in (v) are to be identified with light, and are the cause—together with the structure of our organs—of our perception of colours and shapes. (vii) Just as it is practically certain from experiments with Savart's wheels and with syrens that the pitch of notes that are heard depends on the number of vibrations that reach the ear per second, so we find that many otherwise inexplicable phenomena receive an elegant explanation when a similar assumption is made as to the connexion between perceived colours and periods of vibration of light.

Such, then, in the roughest outline are the arguments on which science founds its particular theory as to the causes of our perceptions. It will be seen that to be plausible the arguments must assume that what is real but imperceptible resembles that which we perceive. *If* primary qualities be real, then it is very probable that what we perceive is more differentiated than it appears to be, and *if* the imperceptible obeys the same laws as the perceptible, then it is very probable that the account which science offers of the mode of production of our perceptions is correct. On the other hand, unless we have reason to believe that, at any rate with regard to primary qualities, the result of this whole complicated process is actually to make us perceive the real, even if it only enables us to perceive a part of what is really there, the whole theory becomes meaningless as it stands, and must either be rejected *in toto* or replaced by some very complicated set of further propositions. Unless, for instance, what we perceive as extended really is extended, it is certainly not obvious what could be meant by saying that the perception of it was caused by the imperceptible motions of imperceptible parts of it. The question, then, as to whether we accept the instrumental view which holds that all this causal process ultimately ends in the perception by the mind of something real, or the purely causal view according to which it merely causes us to perceive an object which is an appearance, and allows no conclusions about the real further than the conclusion that it is able to cause perceptions of this kind, is no academic one, but is vitally important to science. Once grant that science is right in taking the instrumental view with regard to the perception of primaries, and it can go on its way rejoicing, and everything that it tells us will be a real contribution to cosmology. But once deny this and its eminently successful theory will have destroyed its own premises, and the attempt to restate it in terms of the new situation must be laborious, and may well be unsuccessful. But before we come to this vitally important point as to whether science can justify itself in retaining just so much of the instrumental view as it does and replacing it everywhere else by a causal one, we must consider a little more closely the position which science takes, that when we perceive a reality the cause of our perception is to be found in it.

If it be true that the perception of all that is real in an object perceived is caused by events in that reality, the position of science as to the unreality of secondaries might seem to follow logically from its statement that the perception of secondaries is caused by states of primaries. For if '*P* is real' implies that the perception of *P* is caused by events in *P* itself, it must follow that if the perception of *P* is not caused by events in *P* itself then *P* is not real. Such an argument, though formally valid, would be illusory in the present case because it depends on an ambiguity in the word 'in.' No doubt if the scientific theory be correct the perception of red is caused by the motions of imperceptible bodies, and with respect to the causal laws involved, nothing is mentioned except their primary qualities. That, however, is not surprising. The observed laws of mechanics are always stated in terms of primary qualities, simply because it is observed that it is practically[1] indifferent to them what colours the bodies may have. We have no ground for denying that the little particles whose motions cause the perception of a red body may be red. It will entirely depend on whether we hold that the effect of the wave-motion on the eyes and brain and mind is to bring the latter into relation with the real colour or merely to produce a perception with an apparent object, viz. red. Unless you first deny the possibility that the motions, which as a matter of fact cause you to perceive red, are the motions of red bodies, you cannot hold that the fact that without those motions you cannot perceive red, and that the laws of them involve no mention of redness, proves that they cannot be red. In fact, you cannot be sure that the perception of red is not caused by events in what is red until you know that they are events in what has *merely* primary qualities, and therefore you cannot use the scientific theory of the causation of perceptions to disprove the reality of colour.

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[1] Not theoretically, because of radiation pressure.

But is there any reason to accept the scientific view that when we perceive something that is real the events that cause that perception are to be found in the reality that we perceive? *A priori* there is certainly no very good reason, so far as I am aware, for such a belief. When we perceive a definite reality, the event whose causation we are seeking is the establishment of a relation between the mind and that reality. Certainly on the ordinary view of causation this event must have a cause; but I know no reason why that cause should be found in the reality which is *relatum* to this relation. If I throw a stone through the third ground-floor window on the right of the Great Gate, the cause of my throwing it through that window may have no connexion with events in the window or the room beyond. It may just as well be because someone jerks my arm at the time, or because I believe that three is a sacred number. I think that the best reason that science could offer for its belief would be that the assumption of it seems to lead to a general theory that is better in accord with the observable facts than any other that can be suggested, whilst it is not intrinsically improbable if we suppose primary qualities to be real. There is, however, a further reason which makes this suggestion an intrinsically reasonable one. When we perceive a real thing the effect is a setting up between it and the mind of a certain relation. It is held that for this to happen a mind, a certain kind of organ, and an event of some sort are necessary. The question is: What sort of event and where? The reason why an event is wanted at all is because our perceptions begin and end, and we perceive now this and now that. Granted, then, that we knew the type of event required, we should still have to ask why the relation set up by it between the mind as referent and a real object as *relatum* should have now *X* as *relatum* and at another time *Y*. Yet this certainly happens, and therefore it seems as if the nature of *X* and *Y* ought to enter somewhere into the statement of the causal law. Now, of course, it *need* not be the case that the particular way in which *X* and *Y* enter should be that the event is in *X* when *X* is perceived, and in *Y* when *Y* is perceived; but, since the position of the event is as yet undetermined, and since it is certain that *X* and *Y* must be mentioned somewhere in the causal law, it is certainly reasonable as an hypothesis to locate the events in *X* and *Y* respectively, and to say that when a real thing is perceived the event that causes us to begin to perceive that thing is an event in it. When once this is done we build up on that probable hypothesis the scientific theory that the cause of the perception of both primary and secondary qualities is to be found in imperceptible events in imperceptible parts of what we perceive in this particular perception. And in so far as the theory, founded on this initially reasonable assumption, is found to account for actually perceived phenomena under various circumstances, the initially probable assumption has its probability strengthened.

We can now sum up the scientific position and the arguments for it, including those by which it holds itself to be justified in rejecting secondary qualities. (*a*) There is reason to believe that our successive perceptions depend jointly on our minds, the permanent structure of our bodily organs, and on some variable event whose location is as yet undetermined. (*b*) In the perception of the real the effect of these causes is to establish a relation between the mind and some particular reality that is perceived. (*c*) Since we sometimes perceive one object and sometimes another, and this only partly depends on our own volitions, it will follow that, if our minds ever enter into this relation with real objects, a complete law of the causation of our particular perceptions will have to take into account those qualities or relations of the reals in question which determine why the mind is related now to one and now to another. (*d*) Since the position of the event, which is a joint cause of the perception according to (*a*), has not yet been defined, it seems probable that we can satisfy (*c*) by locating it in the particular reality which it causes us to perceive. (*e*) When we make this assumption and try to determine the nature of the events in question, we are led by ordinary scientific arguments to the view that the causes of our perceptions are motions of imperceptible pieces of matter contained within the boundaries that we actually perceive. (*f*) But the laws at which we arrive not only do not require us to take into account the secondary qualities of these imperceptible particles, but also account for the perception of the secondary as well as of the primary qualities. (*g*) Hence not only is the probability of our initial assumption that our perception of the real is caused by events in the realities that we perceive strengthened by the agreement of the theory based on it with the facts, but also, since that theory accounts for the *whole* of what we perceive by events which are only known to have the characteristics of *part* of it, there is no reason to suppose that the remaining characteristics of what we perceive are real.

We must now criticise this argument. It is obliged to hold that primary qualities are real. Its troubles spring from the fact that it certainly does not believe that all perceived primary qualities are real and that it holds that no secondaries are so. A purely instrumental theory or a purely causal theory does not suffer from such difficulties. Their special difficulties are that the one gives us a great deal more reality than we want, whilst the other makes the undoubtedly successful scientific theory difficult if not impossible to state, consistently with its conclusions. ✓

We have already said something about the scientific argument against secondaries. But we merely showed there that the scientific theory, even if fully accepted, does not make it impossible that there should be real secondaries, and that the mechanism of the wave-theory should be the one way by which we are able to perceive them. But the argument in (*g*) above against secondaries is rather different. It is not directed to proving that they cannot be real, but only to showing that there is no reason why they should be so. Let us consider this argument. It simply rests on the fact that, whilst the theory as to the causes of our perceptions of primaries has to be stated in terms of primaries, the theory of the causation of the perception of secondaries does not explicitly demand any more than primaries and events in them. Now we have indeed seen that it is utterly impossible to conclude from such an argument that bodies are not really coloured; but, for all that, it might be held that if we accept the general theory there is a stronger reason for believing in the reality of primaries than in that of secondaries. For unless some primaries are real the theory as stated falls to the ground, whilst on the other hand it would be perfectly consistent with the theory—though not, as is so often believed, a necessary consequence of it—that all secondaries were mere appearances. Still there is one consideration which is relevant and must be noted. We cannot confine the supposed real qualities to extension and figure and number. We only experience extended colours or temperatures or hardnesses, and I fail to see what right we have to apply laws which have been obtained by observing extended bodies that always had other qualities beside extension to mere extensions. I think then that we shall have to conclude that the theory that makes primary qualities real must also accept the reality of some other quality related to extension in the same sort of way as are colours and temperatures. That quality *might* not be a colour or a temperature, or, if it were, it *might* never be the colour or temperature that we perceive. But, as we have seen, there is so far no independent argument to make us take the instrumental view for primaries and the causal view for secondaries, so that when it is once granted that the instrumental view must be taken for at least some primaries there is no good reason for not taking it also for the perception of at least some secondaries, and supposing that sometimes at any rate the result of all the complicated mechanism of the wave-theory and the eye and the brain, is to bring us into direct perceptual relations with real colour.

It ought to be noted that the same positive argument does not apply to sounds. I do not think that it is possible to *prove* that the instrumental view is not sometimes true of them as well as of colour, and that the vibrating bodies and the waves and the complex structure of the ear are not the means by which the mind perceives what would still exist whether perceived or not. But, on the other hand, whilst all that is extended has colour or temperature or 'feel,' all that is extended is not sonorous. Hence there is no positive reason for believing that the perception of sound is any more than an effect of the motions of perceptible bodies, and the perception of a mere appearance.

The crux of the whole question then really is whether we can keep the instrumental view for the perception of primaries. If so we can keep the scientific theory as in essence true about a large part of reality. There being no *à priori* reason why we should not accept the reality of primaries merely on the ground of relativity to an organ, the difficulty springs not from this relativity, but from the fact that neither science nor common-sense believes for a moment that the instrumental theory applies to most of the primary qualities that we perceive. Science is perfectly convinced that most of the shapes and sizes that we perceive are not real, but are appearances more or less like the reality. And here it has clearly dropped the instrumental theory altogether.

We must therefore consider the cases in which it is believed, and those in which it is denied that we perceive real primary qualities, in the hope that we may be able to justify the distinction that is drawn. In fact we have got to answer the question that I asked in p. 200 of this chapter: 'Can we give a reasonable account of what we mean by bodily organs being in proper adjustment, which will enable us to hold that under definite knowable conditions the objects perceived by their means are real, but that under others, such as the pushing aside of the eye or the change of shape due to motion, they are not real?' All the arguments of any weight against primaries practically come to the assertion that we cannot make this distinction. Thus we have to consider our old friends the successive ellipses seen instead of an object which is believed to be really a circle, and the second object seen on pushing aside the eye, and the visions of sleep and drugs. The realist view seems to be that in the main we can distinguish; that, although under many circumstances what we perceive are appearances, yet these can not only be known to be such, but can also be connected with the reality by known laws, so that its qualities can be discovered from theirs.

The idealist argument would be: You perceive shapes and sizes, and as you change your position they alter slightly. You do not believe that all these successive qualities are real and yet you do believe that primary qualities are real. But what ground can you possibly have for your conclusions about real primary qualities except what you do perceive, which you admit to be appearance, and all of whose observable laws must be laws of appearances? With regard to the visual perception of three-dimensional objects the case is still more paradoxical, as Mr Prichard[1] points out in his able book on Kant. We believe that there may be real spheres, yet we are equally certain that *if* there be real spheres what we see when we say, in our crude way, 'that we perceive a sphere' *never* is spherical but is ellipsoidal.

This must necessarily modify the instrumental view of knowledge. It will now not be able to say in general that in the perception of real primary qualities some event in the real makes us perceive that real. In the case of the three-dimensional bodies we never do perceive the real, and therefore, if the event in what we believe to be real causes us to perceive anything, it is at any rate not the real. It will have to be modified somewhat as follows. In many cases we cannot perceive the real object, events in which are the joint cause with our bodies and minds of our having the perception. In fact, in the case of seeing three-dimensional objects we never can perceive the reality. But the characteristics of the appearance that we do perceive depend jointly according to discoverable laws on (*a*) real events in the reality that is said to 'correspond' to the appearance, (*b*) on the real primary qualities of the reality, (*c*) on the real relative spatial positions of the organ and the reality, and (*d*) on the state of the organ and the mind. We must note how very different in principle this is from the old instrumental view. In that view we could perceive realities, and, when we did so, the effect was the establishment of a relation between the mind and a pre-existing reality. No question of likeness of cause and effect arose; for nothing could be more unlike the cause (viz. events in the way of motion of imperceptible parts of the reality perceived) than the effect (viz. a relation between the mind and the reality). But here the question becomes a very pertinent one. If you never perceive the reality but only appearances, which, with the perceptions of them, are caused as wholes by events in the reality, how do you discover those laws which enable you to judge, either (*a*) that the reality has primary qualities as the appearance has, or (*b*) that the particular primary qualities of the reality can be deduced from those of the appearance? Here you do seem to be assuming quite unjustifiably as you did not do on the old statement of the instrumental theory, that one aspect of an effect must resemble the thing in which the events that caused it took place.

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[1] *Kant's Theory of Knowledge*, Chap. IV. *passim*.

Is there any way out of this difficulty? I think the first point will be to investigate its premises a little more closely. The essence of the argument is that with three-dimensional bodies you say that you never perceive the reality, and therefore it seems impossible to see how you can be justified in your further assertion that you know how the qualities of the reality are connected with what you perceive. One important point is that so far the argument has offered no ground for doubting that extension and figure of *some* kind are real. I may shift about from place to place keeping my eye (as we say) on the same object, and I may observe that it changes in shape; but I never observe that it ceases to be extended and to have a shape. Now it seems to me that this is a very important fact for the present purpose. We know that if primary qualities be real at all, they cannot just be shapes in general and sizes in general, but must be particular shapes and sizes. Moreover we have now seen that the present argument at least gives us no reason to doubt the existence in reality of the general quality, and therefore of some (though, it may be, unknown) particularisations of it. Hence if we find general laws connecting the shapes and sizes of *perceived* figures we may be able without undue presumption to gain some knowledge of the shapes and sizes of *real* figures that correspond to what we perceive.

Before, however, we work out this consideration any further it is right to notice an alternative that commonsense and science do not accept, and, as far as I know, do not discuss. This is the alternative that the circles and ellipses really do alter in shape as we move relatively to them. After all why should not our movement be a cause of their change of shape according to definite laws just as well as our squeezing them with our fingers if they chance to be made of some bendsome material? In that case it is clear that the purely instrumental theory would be saved. We should perceive something different in each position, but then the real object would have changed. This would be a very reasonable view to take if we happened to be alone in the universe, but it will not do in view of the fact that there are other people who claim to perceive the same reality as ourselves. For if what both of us perceive is a reality, and if it really alters when we move, then, if I move and the other man stands still, the same reality will both change and remain unchanged. Thus the main reason why we cannot accept this explanation is not that there is anything intrinsically improbable in it, and not that it would not perfectly well account for the changes of the objects perceived with change of position for each one of us taken separately. The trouble about it is that it will not allow us in any sense to say that two people 'perceive the same thing at the same time' no matter how much alike the objects of their perceptions may be. For the theory in question will make the objects of all the successive perceptions of each of us real, and, since the above considerations show that they cannot in general be the same realities as other people perceive at the same moment, we shall need as many realities as there are people. And this common-sense wishes to avoid.

The suggestion then that the real actually does alter in shape as we move about will not help us greatly, and, having mentioned and discussed this suggestion, we can return with a good conscience to the problem of how much, if any, of the instrumental theory of perception can be kept when once some of it begins to be rejected. So far we have seen that no reason has been adduced even by those who say that we never can see the true shapes of solid objects, to prove that we are not right in ascribing primary qualities to the real even if perception does not at once tell us what the particular primary qualities may be. The next point to notice is that science does not believe that in *no* case do we perceive the real primary qualities of objects. For it holds that in the case of two-dimensional ones at any rate there are positions from which the true shape can be seen.

Let us consider then whether there is any reasonable ground for distinguishing those cases in which we are said to perceive the true shapes of bodies from those much more numerous ones where this is denied. We will take the circle and the ellipses. All the ellipses are said to be appearances and the circle alone is said to be real. Are there any really relevant differences between the circle and the ellipses that will justify this distinction? The facts of the case here are that (*a*) in one set of positions and in one set alone we perceive a circle, and (*b*) in all positions, whatever we may see, we feel a circle. Now we have already argued in our second chapter that, apart from some kind of causal theory, it is a sheer fallacy to suppose that the agreement of two senses increases the probability that that in which they agree is real. We are no more certain that what we feel is circular than that what we see is elliptical at the same moment;—we could not be *more* certain, for both judgments have the greatest certainty that it is possible for judgments to have. Hence it is perfectly open to us to say either that both the seen ellipses and the felt circle are real and coexist; or that both are mere appearances; or that one is real and the other an appearance; but, so far as I know, there is no reason why what is perceived by two senses should be more likely to continue to exist when it is perceived by neither than what is perceived by only one. This was the conclusion that we reached before we elaborated the instrumental view of perception as we have been trying to do in this chapter. The question now is whether that view adds anything to the results of our earlier discussion. I am inclined to think that it does. We must remember that we are now accepting the *possibility* of the instrumental view—which has never I think been successfully denied—and that it is our present business to see if there be any ground for retaining it at all when it has been restricted as much as science and common-sense restrict it. Now, on that view, if we ever do perceive a real object, our minds are brought into direct relation with that reality owing to events that occur in it, and the possession by our minds of proper instruments. Now it is quite certain that the *same* real object cannot be at once circular and elliptical. Hence if our minds are ever brought at the same time into the relation of perception to an object that is at once circular and elliptical it cannot be real. Hence if we suppose that the tactual and visual shapes belong to the same object, none but the circular object can be real; for in all other cases there is this synthetic incompatibility. Thus, whilst it is perfectly true to hold as we did in an earlier chapter that agreement between the deliveries of various senses is no proof that the object is more than an appearance, yet we can say that if the mind *ever* comes into perceptual contact with the same reality through two different senses it is only where their deliveries agree that this can actually be the case. When there is disagreement either one sense is not in contact with reality at all, or they are not in contact with the same reality. We have already discussed in the second chapter in what sense we believe that visual and tactual spatial characteristics do belong to the same object and can be synthetically incompatible.

I think then that the position which common-sense takes up as regards the ellipses and the circle is a perfectly reasonable one. The agreement of the senses does not indeed, as we have seen, refute the possibility that the felt and seen circle may both be appearances. If we decide then (*a*) that most of the visually perceived objects are to be counted as appearances so as to prevent the infinite multiplication of realities, (*b*) that all the visual objects and also the tactual objects are connected with a single reality, and (*c*) that under suitable circumstances this common reality can be an object of both sight and touch, we shall have to conclude that the reality is circular and not elliptical. To take any of the ellipses as real, together with assumptions (*a*) and (*b*), is (1) arbitrary, since it is only the circle that is marked out by a special property, viz. the agreement of the senses, and (2) involves the view that touch does not here bring you into contact with reality at all, though sight does.

But once the distinction has been made, the laws that we can discover to connect the objects of our perceptions as regards spatial qualities will in certain knowable cases be laws connecting realities with appearances. For instance, when we have distinguished the circle as real from the ellipses as appearances, the observations that we can make on the perceptions that we have when we stand in certain places, will teach us laws connecting real primary qualities with those of appearances. And when these laws are once firmly established we shall be able to argue from an object of perception to the most probable particular primary qualities of the reality that 'corresponds' to it.

Now our ability to do this was precisely the paradox which was so troublesome before, viz. the question of how it can be possible, for instance, to say that we know that all our spatial perceptions have merely apparent objects if these be solids. We can now easily justify this apparently paradoxical statement. We have no reason to doubt now that we can perceive the real figures that exist in two dimensions and distinguish them from mere appearances. By so doing we can discover the laws that connect the shapes of appearances seen from certain positions with the shapes of realities. Then we can give a meaning to the saying that we know that we can never see what is really a sphere. This means that when we feel a sphere we always see some kind of ellipsoid no matter in what position we stand, and when we see a sphere from some definite position, we can always only feel an ellipsoid. And the general laws that we have discovered by the consideration of the changes of shape of two-dimensional objects with changes of position will enable us to understand why this should be the case.

So far so good then. It was essential to the scientific theory to be able to keep the instrumental view of perception with regard to primary qualities, and there seemed to be a grave doubt whether it could do this in view of the fact that for all secondaries and for many primaries, it held a different theory which made them appearances. We saw that in regard to secondaries there just was no conclusive ground for a distinction, though it would not make any real difference to the scientific theory whether it dropped them or kept them. And, with regard to the changing shapes of what we see, most of which science pronounces to be appearances more or less like the reality, we saw that (*a*) this distinction might be drawn without our having to deny that shape and size are qualities of the real; (*b*) that it is possible to agree that if the instrumental theory be *ever* valid, it will be so only when science holds it to be so, and not when it denies that it is; and (*c*) that by applying this criterion and starting from the perception of objects of two dimensions, science can discover laws connecting real shapes with those of appearances seen under definite conditions, and come to the conclusion that we never see solid bodies as they really are.

But we are by no means at the end of our troubles yet. All that has been said in the last paragraph will hold if we have reason to believe that the instrumental theory is a true account of perception in at least some cases. But the question is whether there are not other considerations beside those that have already been considered that render this very improbable. In fact the question will be: Can we give any reasonable account of what we mean by the instrument being wrongly adjusted or out of order; and will not the account that we have to give of this be so general that it will replace the old instrumental theory altogether?

Let me now try to make the problem a little plainer. The difficulty arose most clearly over what common-sense holds to be sheer illusion, though it will be easy to see that it also applies to the case of the appearances that are called 'appearances of' a reality like them such as the successive ellipses. The position is as follows. Grant that there is illusion whether small or great and you must grant that the complex mechanism involved in perception can produce two entirely different results. Entirely different in one sense and yet on the other hand unfortunately very much alike. It is the combination of their extreme likeness and their utter difference that threatens to wreck the instrumental theory, and with it, the science of physics as ordinarily understood. When we perceive reality, if we ever do so, the effect of the whole process in the reality, the organ, the brain, and the mind is to establish a relation between the mind and the reality that we perceive. When we perceive appearance, the effect of much the same process in the organs and the brain is to produce, not a relation to something already existing, but a whole of object + relation to mind. Now two effects could hardly be more unlike than this. Yet on the other hand there is an immense likeness between them. The perceived object in both cases is very much the same. The ellipses that are only produced as elements in the whole called a perception are extremely like the circle which is believed to be able to exist out of the perception. Hence it is not unreasonable to say to the person who wants to keep the instrumental view in one place and to drop it in another: Can you really believe that practically the same mechanism can produce such utterly different results? Again, you have granted that *most* of the objects that you perceive are appearances. You only see the circle which you believe to be real in one set of positions; but you see the ellipses which you believe to be appearances from an infinite number of sets of positions. Surely it would be more reasonable to hold that it too is an appearance. Look at the advantages that will accrue from this slight change. At once you will be able to drop this incredible belief that the same mechanism sometimes brings your mind into relation with a previously existing object and sometimes creates the object in the relation. And, with the dropping of the difference, the likeness which was so puzzling on your original theory will become natural. In all cases the effect will be the production of a perception as a whole whose object cannot continue to exist out of it. What then more likely than that some common cause under slightly varying conditions produces these very similar perceptions?

This is the argument for idealism as against the instrumental theory of knowledge when an effort is made to confine the latter to some parts only of perception. It offers common-sense and science a choice between the incredible view which they agree in rejecting that anything that anybody perceives anywhere is real and the [illegible] view which it has just attacked. It concludes that realism can keep to neither position and must pass over into complete idealism. The argument seems to me to be a powerful one, but the results of accepting it are serious. All that we can perceive is appearance, and there is no reason to suppose that the real has any qualities like those which we perceive. All that can be said of the real is that it must be of such a nature as to be able to cause the perceptions that people actually have.

We must consider this argument as carefully as possible and see if it be sound. The line that it takes is that we must suppose that in many cases the effect of the processes in our organs and brains is to set up a perception as a whole instead of a relation to a real object, and that therefore it is better to assume that this is what happens in *all* cases of perception. In fact the present position is that the causation of our perceptions can be analysed vertically into states of brain, caused by states of organs, caused by states of something else. The states of brain, however caused, produce the same perception whose object is of course an appearance; but in some cases the object perceived resembles a reality, states in which are a remote cause of those in the organ.

The first question is whether this view can formulate its position consistently with its own conclusions. We must remember that we are still left with all the old observations, viz. those that tell us that without an eye colours are not perceived, and with an eye the possibility of perceiving various colours depends on the internal structure of the eye. But, whilst it was easy to formulate these conclusions on the instrumental theory, it is less so now that we believe that all objects of perception, and therefore the eye and its structure, are appearances. I think however it can be done. I know that when it is impossible for me to perceive an eye in a man's body under circumstances where I might expect to do so, that as a rule he cannot perceive colours or visual extension. On the causal theory the ultimate cause of my perceiving an eye is certain states of my brain. A remoter cause is states of my own eye, and a still remoter one is some other states of the real. Now it is clear that by all these conditions I do not mean perceived conditions. For it is certain that people perceive colours when no one perceives their eyes or their brains. Hence we must put the whole theory into terms of real causes of perception. I think it will then become reasonable to state the observed facts in the following way: There is a relatively permanent structure in reality, which is a condition of the possibility of the mind which is connected with it, perceiving coloured and extended appearances, and which is also a remote cause of other people, whose minds are possessed of similar real structures, perceiving an eye. We can deal in the same way with correlations between the perceptible structure of an organ and the possibility of perceiving some particular quality like red or green.

Thus it is hardly true to say that a purely causal theory leads to complete agnosticism, for it does allow of a certain amount of analysis of the causes of our perceptions in accordance with the actually observed conditions for their production. We must now see whether such an analysis cannot be carried further. If we consider the general nature of what we perceive we shall see that all perceptions have much in common. They are all built up out of quite a few general qualities like colour, sound, extension, figure, taste, smell, temperature, etc., which continually recur. The differences other than geometrical between our objects of perception consist in the following respects: (1) The presence of a given general quality in one and its total absence in another; (2) Difference in the degrees of the characteristics of the qualities. By this I mean that the general qualities like colour have for their particularisation a small number of characteristics which may be compared to independent variables—e.g. colour is particularised by colour-tone, saturation, and intensity. Each of these variables has a continuous set of values, and perceptions differ in their objects according as the values of these variables differ. Apart from characteristics and their continuous series of possible values there would be only $2^n - 1$ different possible objects of perception where n is the number of different general qualities and is, as we have seen, quite a small number. But all these general qualities have to be particularised by the special values of their characteristics. Of these they generally possess several—thus colour and sound have three apiece—and each of them is generally supposed to have a continuous or at any rate a compact series of possible values. It is from the last fact that the infinite variety of the objects that we perceive springs.

Now this analysis of what we do perceive at once suggests, it seems to me, a further analysis of the causes of our perceptions. We have now seen that we have very good evidence for the belief that the possibility of having perceptions, whose objects have the general qualities that we have mentioned, depends on the connexion of the mind with a real structure, states of which are the condition of one mind perceiving these general qualities, and other states of which are the cause of other minds connected with similar structures perceiving bodily organs. Hence the general qualities perceived can be correlated with this permanent real structure without which they cannot be perceived.

But, on the other hand, we are not able to perceive in our organs anything that corresponds to the particular values of the characteristics of these general qualities, which perpetually alter as we have different perceptions. But it seems not unreasonable to believe that what determines the perception of a given *particular* quality at a certain moment must be states in that permanent structure which we believe to be the condition for the perception of the *general* quality of which this is one possible particularisation. This, at any rate, seems the most reasonable analysis to make.

We must not, however, make the mistake of supposing that we can go on to conclude that if a particular quality has (say) three independent characteristics, there must be either three real states in its cause or that, if there be only one state it must have three distinguishable characteristics like e.g. wave-length, amplitude, and form. For we saw in the chapter on Causality that, although a different effect—however small the difference—must have a different cause, yet this does not prove that, when the differences in the effects are differences in the values of certain independent variables with a continuous range of values, the cause of any given effect of the kind must be as differentiated as that effect. We cannot, in fact, be sure that the three characteristics of perceived colours may not be due to events that have only one variable characteristic.

I think we can now say that we have been able to show that a purely causal theory, which makes all the objects of perception appearances, can be stated compatibly with the observations about our bodily organs with which the two rival views of instrument and cause both started. We have seen what sort of analysis can probably be made, on the strength of those observations, of the unknown causes of our perceptions. But the difficulties attending the scientific theory of the causation of perceptions on this view, which makes the objects of all our perceptions appearances, remains untouched. What is the objection to the scientific theory from our standpoint? It is a theory of the more remote causes of our perceptions, of the causes of those states in our organs and minds, which, however caused, end in a perception of an appearance. But it is stated in terms of primary qualities and it actually involves the view that we can find out the primary qualities of these remote causes in many cases from those of the appearance perceived. Hence it assumes that the remote causes of our perceptions resemble their objects not only in the general way that both have primary qualities, but also in the much more particular one that there is a general resemblance between the shape of the appearance and the shape of the remote cause. We might put the matter thus: Real primary qualities are essential to the scientific theory, therefore (*a*) the causes of our perceptions are held to have primary qualities and must be real, since they are admitted to be imperceptible, and (*b*) the theory can only be built up by assuming that the causal laws between these remote causes and the events in the real structure of our organs are those that hold between observable primary qualities. On the other hand the conclusion of the theory gives us no warrant whatever for supposing that primary qualities are any more than appearances.

And finally it seems a very extraordinary thing that science should hold that what resembles the object that we perceive is not its immediate cause, or the last cause but one, but a cause which, on its own theory, is a long way back in the vertical causal series.

The first point that we have to notice is that science holds that in general we only perceive an object which something in the real resembles, when other people also perceive it or can do so. There are difficulties about this view which we will discuss later; at present we will accept it. Now we notice that under similar circumstances a slight change in our own position, when we remain as we say, 'looking at the same thing,' makes but a very slight change in the qualities of what we perceive. The ellipses are very like the circle and each other. Now this I think quite reasonably suggests that under such circumstances our perceptions have a common remote cause, and that some slight change of condition makes the difference in what we perceive. This is the sort of justification that science can offer for the apparently arbitrary procedure of fleeing to a remote member of the causal series as the one that is supposed to resemble the object perceived. It does not of course justify the assertion that the object perceived is like any of the causes of the perception; but it does suggest that, if it were, it would be a cause remote enough to be common to the causal series of several observers that it would resemble.

The next point on which we must be clear is that the further determination of the real world does not pretend to be anything more than hypothetical. This, it must be remembered, would be true of such arguments and conclusions as are involved in the wave-theory of light and the dynamical theory of gases even if we could have maintained the instrumental theory, and held that we sometimes perceived the real. For, although on that theory we perceive the real, it is quite certain that we do not perceive those particular realities with which the scientific theory deals, and therefore they can only be proved by the success with which they account for what we can perceive. Now, the only reason that we are worse off when we believe that we do not perceive the real is that the initial probability of an hypothesis about the real, stated in terms of qualities of what we perceive, is less. The probability that is reflected back on such an hypothesis by the fact that it does explain what we perceive remains the same as before. But it is easy to see that, under the circumstances, any alternative hypothesis about the real will have to rest its probability entirely on its ability to explain the perceived. For by our own admission nothing is directly known about the qualities of the real and therefore the initial probability of any further particular determination of it is the same as that of any other, so long as the laws of logic and probability are obeyed by it. Hence in comparing the probability of any two alternative theories as to the further determination of the nature of the real causes of perception we need not consider anything but their respective success in explaining what we do perceive. And there is certainly no alternative theory of the nature of the real before the public at present that can claim to explain so many of the facts so well as the theory of science. Thus the correct answer of science to such an objection as Dr McTaggart makes in his *Dogmas of Religion*, that 'the existence of matter is a bare possibility to which it would be foolish to attach the least importance[1],' is that, whilst all alternative possibilities are equally bare when you start from the view that what you actually perceive is appearance, this particular one accounts for the perceptions that we actually do have in a way in which no other, so far as we can tell, does. (I have already discussed in my chapter on Phenomenalism the plausible theory that the causes of our perceptions are themselves mental in character.)

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[1] *Op. cit.* p. 95.

But the statement that, on the theory of probability, if we start from the view that all that we perceive is appearance, the further determination of reality in terms of primary qualities which science offers is the most probable with which we are acquainted, must not be misunderstood. Science has absolutely no need to suppose that reality has those peculiarities which I have already mentioned as being incommunicable even from man to man. I mean the peculiar and incommunicable sense experiences that we have. It is true that we cannot perceive reality and therefore when we say that there is a real square we cannot mean that the reality has that sensuous particularity that we experience when we see a square. But this is utterly unimportant to science, because, as we have seen, such sensuous particularity is absolutely incommunicable, and therefore cannot be a possible subject of a science of any kind. Just as we cannot possibly know whether *A* and *B*, who always use the words 'red' and 'green' under the same circumstances, really have the same experience, so we cannot and do not need to know whether the real has the peculiar sensuous quality that we experience when we use those terms. But just as we can agree with each other as to the use of these terms—the important point being that we all find and fail to find distinctions in what we perceive in agreement with each other—so we can ascribe primary qualities to reality in the sense that we can believe that it will agree in having distinctions where we perceive spatial distinctions and where other people agree with us in perceiving them. This, then, is all that can be demanded when we say that the real has primary qualities; we cannot mean more than when we say that the objects of other people's perceptions can be known to be square or red, and we know that this merely means that one of us will never find homogeneity where the other finds distinction.

If, then, we allow science to assume hypothetically the reality of at least some primary qualities, we have seen that it can build up a very successful theory to account for the perception both of primaries and of secondaries. It does this, as we saw, by assuming that the real shapes are those for which sight and touch agree, and therefore that in some cases at least, though very few, the appearance is exactly like the reality, events in which are a remote cause of our perception; whilst in general the perceived distance of a visible object is the real distance of the remote cause. But it would obviously be very much better if the scientific theory could be stated in a much less definite form. If, in fact, we could state it in terms of causes without having to specify their nature so accurately, it is clear that it would still retain such probability as it does from explaining the facts, but would gain in probability by not having to make such definite and complicated assumptions about reality. The question is whether this is possible, and to this question we now turn.

It is tolerably easy to offer an account of the scientific theory of the causation of our perceptions of sounds which does not assume the reality of primary qualities. The ordinary arguments assume this reality and proceed from the fact that when we begin to hear a sound there is strong reason to believe that we could always have perceived a body vibrating which began to vibrate at a time before we began to hear the sound. This time is shown to depend on the perceived distance of the vibrating body and the nature of the medium between it and our ears. Now, on the present theory, to say that we always could have perceived a body vibrating at a certain time, means that a real thing existed, states in which are capable of making us perceive such a body. We know that there must be such real causes on the present theory; we know, too, that they are remote causes, events in which produce real changes in that real structure which 'corresponds' in the sense discussed earlier to our perceptible ears. But we do not need for the present purpose further to specify the nature of that real thing. Hence we can state the scientific law as follows: A remote cause of our hearing a sound is a remote cause, events in which are capable of making us perceive a vibrating body if there be a proper organ in a proper state at the time. Thus the theory adopted by science for the cause of our perception of sounds, though stated in terms of primary qualities, can be stated just as well in terms of the supposed remote causes of our perceptions of primary qualities. It then becomes indifferent to us whether primary qualities be realities or merely appearances, and science does no harm in continuing to state its position in terms of primaries, since we can always interpret it in the way in which we have just been doing.

But clearly this will not help us very far unless we can also state the scientific theory of the causation of our perceptions of primary qualities, and of colours in terms that do not assume the reality of primaries. And this is by no means a simple problem.

Let us begin by stating the scientific theory in terms of primaries, and then see if it be possible to transform it. The theory is that the last cause of our perceiving an object of a definite shape and colour is states in our brains; that the cause of these is states in our eyes; that the cause of these is vibrations that travel with a definite velocity through an imperceptible medium; and that the cause of these is the vibrations of numbers of imperceptible particles. This last stage of the causal process is supposed to have a definite figure closely related to that which we perceive as a result of the whole process. Moreover, the little particles, beside lying within this definite boundary, are supposed to have a position in space relatively to us, which is practically that in which the perceived figure is seen to be. Consider now for a moment the difficulties that beset any attempt to state such a theory as this in terms which do not involve the reality of primary qualities. There are three difficulties about this theory over and above those which occur in the physical account of the perception of sounds. (1) With sound we could under favourable circumstances perceive the vibrating body and the medium. Here we cannot even theoretically perceive either the particles or the medium or their motions. (2) The theory of sound does not try to explain the causation of our perceptions of sounds, which it grants to be appearances, by the states of supposed real sounds. But this theory does undertake to explain the perception of primary qualities by real primary qualities and their states, whilst it is forced to conclude that, so far as we can tell, there is every probability that all the primary qualities that we perceive are appearances. (3) As a consequence of this difference the theory of sound did not have to come to the amazing conclusion that a cause *four* stages back in the process closely resembles the object of the perception, which the subsequent stages end by producing. But this is the conclusion at which the theory of light arrives, or, perhaps we ought to say, from which it starts. Now these three difficulties, peculiar to the theory of light, end in another paradox, which seems almost insoluble except on the assumption of the reality of primary qualities. Science tells us that the remote cause of our perception of an object is imperceptible parts of that object. But it also holds that the object perceived is an appearance; for we perceive chairs and tables as homogeneous and not as quantities of little bodies in rapid motion. But it is hard to see how a reality can be part of an appearance, even if we grant that these can be unperceived parts of an appearance at all. The solution of the paradox seems to depend entirely on the assumption, not merely that primary qualities are real, but also that, as far as distance and shape are concerned, the primary qualities of the appearance that we perceive are substantially similar to those of its remote cause. It then can be said that the little particles are unperceived, and that they form part of a reality which occupies practically the same place as the perceived appearance. But this solution does not seem possible without these very special, and, so far as I know, intrinsically groundless assumptions.

We must not however despair, but see if we cannot state the essence of the scientific theory of the perception of primaries without assuming their reality. We have already seen that the fact that all persons under certain circumstances perceive much the same objects—e.g. ellipses—renders it very probable that there is a common remote cause of their perceptions. Let us revert, then, to the case of the circle and the ellipses. We know that in whatever position we may be, so long as we touch the object, we feel something circular. The question of whether we touch it or not depends on our own volitions. Thus we seem to be justified on the causal theory in supposing that there is a permanent reality which, whenever we will, will give us the same tactual sensations of shape. We know further that we can often get these tactual sensations when we cannot see anything at all. This suggests that the cause of our seeing the circular or elliptical object, when we do see it, is events that do not always take place. 'In the dark,' as we put it, these events are presumably not taking place. But when they do happen we perceive an object that is geometrically very much like what we feel, and, under certain circumstances, is exactly like it as far as shape is concerned. Now this, I think, suggests that there is some permanent reality which causes our sensations of touch whenever we will, and that sometimes events happen in that reality which cause us to see objects of various shapes which, however, are always like the felt shape.

Now the question is whether we might not be allowed to assume the instrumental theory for touch even though we have had to drop it for sight. Let us recall for a moment why it seemed unlikely to be true for sight, and see whether the same argument will apply to touch or not. There was nothing ~~intrinsically~~ impossible or even improbable about the instrumental theory to begin with. It remained a perfectly possible ~~alternative theory~~ to the causal one, which makes all our perceptions appearances, until we come to the fact that science and common-sense agree ~~in believing that~~ the greater part of our visual perceptions, even of primary qualities, ~~are mere appearances~~. Then it had to hold that precisely the same sort of causal processes in the same bodily organs must be capable of producing two entirely different results, viz. the establishment of a relation between the mind and a reality, and the production of a whole perception consisting of an apparent object very like the one that is supposed to be real + the same sort of relation to the mind as we had before. And we agreed that this was a very unlikely state of affairs, and that it seemed much more reasonable to suppose that in all cases the effect was of the same nature, viz. the production of the perception of an appearance as a whole. Thus the ground of the decision against the instrumental theory in the case of sight was that it was held that most of the objects perceived by sight were appearances. But this conclusion does not prejudice the possibility of the instrumental theory for other organs of perception. If with them, e.g. with touch, we were to find no reason for supposing that any of the objects perceived were appearances, we should no longer have the paradox of the same causal process producing two utterly different effects, and therefore no ground for rejecting the instrumental theory as regards the deliveries of that organ.

Now it is at least obvious on the face of it that there are very much fewer cases of perception of objects by touch that are declared to be illusory than there are of sight. For instance, all the difficulties that arise from change of shape with change of position vanish at once here. So long as we feel the object at all, no matter who we are or where we are, we feel the same shape, or at worst can explain the change of shape by a real change in the object. Our feeling it or not depends on our volition, but when we once feel it we always feel it as having the same shape.

But it will be said: There are illusions of touch, and even though there be much fewer than of sight, that is enough to wreck the theory that the organs of touch act purely instrumentally. Let us then consider these supposed illusions and see whether they be really relevant. The two following seem to be typical cases. (i) There is the old Aristotelian experiment of holding a pen between the crossed first and second fingers so that it touches the back of the former and the inside of the latter. Under these circumstances it is said that two pens are felt instead of one. (I cannot get the illusion myself, but I am prepared to believe Aristotle and the numerous other distinguished persons who have been more fortunate than I have.) (ii) The other is the illusion that has been pretty closely investigated in modern times—of the two compass points which have to be placed at different distances apart when applied to different parts of the skin in order to be felt as distinct. Everyone is acquainted with a particular application of this who has been so unfortunate as to have a hole in his tooth. This will appear enormous to his tongue and tiny to the eyes of himself and his dentist.

I do not think either of these criticisms need prevent us from holding that touch is a sense that acts purely instrumentally, and that for aught we know to the contrary, tells us the real shapes of real things. In the first place, so far as I know, there are not supposed to be any illusions about felt shape. The two illusions mentioned above rest on the fact that a judgment in the one case about number and in the other about distance and length, conflict with judgments based on the deliveries of sight; and in these cases we accept sight. The first point to notice is that the experiment with the compass points is entirely irrelevant to the possibility of the instrumental theory. It does not make against that theory if the use of the instrument does not tell you the whole truth about the real; the troubles arise when it gives you a perception with an object that is believed to be an appearance. Now, if there be really two compass-points, and under certain circumstances you only feel one, there is no apparent object involved, and therefore no argument against the instrumental theory. You explain the facts at once by saying that the skin on your back is not nearly such a delicate instrument for perceiving the real as is the skin on your tongue-tip, and therefore distinctions in the real can be discovered by one which are left unnoticed when the other is employed. And if you pass to the further experiment by which it is found that at different parts of the skin, when the compass-points are far enough apart to be distinctly perceived in all cases, the distances perceived by the eye and the skin do not agree, this still is not relevant.

We do not here have at some points, or at all, illusory objects of perception. At all the points we perceive distances between the two compass-needles, but when we compare their magnitude, that at one point seems greater than that at another, although we do not believe that this is really true. Now, comparison of a past perception with a present one in respect of the magnitude of one of its characteristics is quite clearly an act of judgment and not of perception. Now there is no reason on a purely instrumental theory why you should not make erroneous judgments about the real. Let us be quite clear about this point. When I have a perception on any theory, certain of the judgments that I make about its object must be infallible, e.g. judgments as to its being extended, and having such and such a colour or shape. Now, whilst the judgment that it has extensive magnitude and that its boundary lines have length, is infallible, it is quite clear that the perception does not guarantee infallible judgments as to what those lengths are. For you could at best only have infallible judgments as to one length being greater than, equal to, or less than another in the same object of perception. Now it is clear that the present experiment with compass-points does not make us suppose on the instrumental theory that now one of two lines in the real is longer and now shorter than the other. It only tells us at best that if we can trust our memories it seems likely that the length that we now perceive would be judged to be greater than the one that we perceived when a different part of the skin was touched, if as a matter of fact both lengths could be perceived at once, which they cannot. But when once we grant that there is no question in either case of a positive perception of something that is believed to be an appearance, but merely the question of a judgment of comparison between a characteristic in an object perceived by two different instruments we have no difficulty in accounting for the facts compatibly with the instrumental theory that in both cases what we perceive is the real object. Judgments of comparison between the object of two different perceptions may very well be determined by other causes besides what is actually perceived. We may perceive real distance and perceive it as having a definite magnitude, but the actual magnitude which we judge it to have may well depend on other circumstances beside the perceived distance. Hence the same distance may well be judged to have different magnitudes when perceived by different instruments. Nor indeed have we far to seek for the cause of our judging that the magnitude differs in the two cases. In both cases the instruments used are parts of the skin. We know from the earlier forms of this experiment, which I have already discussed, that some parts of the skin are more delicate instruments than others in that one part can discover distinctions when the other only finds homogeneity. Hence it is clear that in the less sensitive parts of the skin, when the distance is really the same, the real state of affairs will be more like that in which no distinction can be detected than it is at a more sensitive part. And this may well be interpreted as a real shortening of the distance between the points. Thus I think that we are justified in holding that, as far as the compass-point experiment is concerned, there is no reason for supposing that what we perceive by the sense of touch is ever an appearance, at least as regards geometrical qualities. And this leaves it perfectly open to us to assume the instrumental theory for this sense. We have not indeed *disproved* the causal theory which makes the objects of all our perceptions appearances, but it removes the sole ground which led us to prefer that view in the case of sight. The causal theory for touch remains a possibility which it would be foolish to adopt without necessity, since with the instrumental alternative we can accept substantially the theories of physics, whilst with the causal one it is doubtful if we could transform them so that the same evidence still supported them.

It now remains to say a word about Aristotle's experiment, and to show that this, too, is erroneous judgment, and not the perception of an appearance, as is the seeing of ellipses when what is real is a circle. The first point to notice is that two instruments of perception are used on the instrumental theory—the outer part of one and the inner part of the other finger. The second point is that if the deliveries of the sense of touch can be trusted, which of course on the instrumental theory they can be, these two organs are not in contact with the same part of the reality. These two facts, which are true on the instrumental theory, can now be used to explain the illusion compatibly with it. No one in the position in which the fingers are held ever does have a *perception* of a single round surface. He feels, on the contrary, two bits of round surfaces. But the assertion that we perceive a pen means that we perceive the whole circular surface, and this we do not do. Hence under the circumstances any assertion about pens, and therefore about the number of pens, is not a judgment based entirely on a perception and guaranteed by it, but is either an inference or an association. It is as much an inference to say, under the conditions of the experiment, that you hold one pen as that you hold two; the only difference is that the latter inference is liable to be made and that it happens to be erroneous. Thus we do not perceive an appearance, but we make a false judgment. And so this attempt to show that we sometimes perceive appearances by means of touch seems to have broken down.

Touch, then, we may conclude, for anything that we have yet learnt, is a purely instrumental sense which always makes us perceive the truth about the geometrical qualities of real objects, even if it be only a small part of the truth and if we be liable to base unwarrantable inferences on such partial knowledge. But if once this position be granted, we have made an immense step forward in the rehabilitation of the scientific theory of the causation of visual perceptions. For we can now agree with science that the geometrical qualities of the appearances that we see bear a close resemblance to those of certain reals, and that in some cases they are exactly like them. And science will now be justified, if it finds that view conducive to the explanation of our visual perceptions—as it does, in supposing that a remote cause of its perceptions is in many cases events which are imperceptible in these tangible reals that resemble the objects of visual perception. For the recognition of the reality of the tangible carries with it a solution of the paradoxes mentioned on p. 252. For instance, there is now no difficulty about real, but imperceptible events in imperceptible little bodies which occupy the same place as that in which the body is perceived to be. For it was granted that touch did not tell us the whole truth about tangible realities. But it was claimed that it told us, so far as we know, nothing but the truth about their geometrical qualities. In that case they are truly extended, and may well have parts too small for our senses to perceive. Again, it now ceases to be arbitrary to take the fourth stage backwards in the causation of a visual perception for that which 'corresponds' to the object perceived. For now we have reason to believe that there can be reals which closely resemble the object perceived in shape, and that events in them may be the common cause of similar but slightly different visual perceptions in various people. It is clearly not arbitrary to take as the corresponding reality that which resembles in geometric form these perceptions' objects, and is at the same time the common remote cause of the perceptions.

At the same time I should hesitate to lay too much stress on the instrumental character of touch. It is evident that in order to be able to maintain it we must be prepared to assert that we *never* perceive appearances by touch, at any rate, as regards figure. We must be able to prove of any suggested case that it is either a perception of fewer, but not of different distinctions from those of the real, or that it is not a case of perception of appearance, but of erroneous judgment based on the perception of the real. Now it would certainly be rash to maintain that no cases do arise which are only explicable as perceptions of appearances. If, for example, we ever have tactual experiences in dreams it is doubtful whether they could be explained otherwise than as the perceptions of appearances. I do not know that we ever do feel the shapes of things in dreams, but a theory that rested on the assumption that we never do so would be in a precarious state.

Fortunately, however, by combining the facts of sight and touch it is possible, even on the assumption that the causal theory holds for the latter sense as well as for the former, to gain some probable conclusions about the real causes of our perceptions of appearances. It is granted that the approximate agreement of our visual perceptions under given circumstances suggests a common remote cause for them. Further, the fact that practically always we all feel the same shape under these circumstances if we will move our fingers along the edges, makes it almost certain that there is a common remote cause of our perceptions of tactual figure. And this is found to be independent of the relative position of the object touched and the body of the person who touches it other than the actual touching organ. Again, there is always a general agreement in shape under these circumstances between what we feel and what we see, and under some circumstances a complete agreement. Yet the processes of causation of the two sorts of perception are widely different. They employ different organs, and one set of perceptions can be had in the absence of the other. Now the fact that when we feel an object we can nearly always see an object of much the same shape tends, I think, to show that there is probably a common set of conditions which the causes of the two kinds of perception obey. When we add to this the facts that there is every reason to believe that the intermediate stages in the causal process are widely different in the two cases, and that something that we have previously only felt may subsequently be accompanied by a visual appearance (owing, e.g., to its being heated or illuminated), we shall be inclined to believe that the supposed identity of conditions in the remote causes of our corresponding perceptions of sight and touch are not identical events. If, then, the events differ, as they must do, since the intermediate causation is so different, and since one sort of perception can be had without the other, we must look for the identity which is to explain why, when both sorts of perceptions do occur together, their shapes do so closely correspond in something else than remote *events*. It looks, in fact, as if we might find it in some *permanent conditions or relations*, which both the events that cause the visual perceptions and those that cause the corresponding tactual ones obey. Now it seems to me that the simplest way to account for all these facts is to assume a real counterpart to tactual perceptions whenever we can share those perceptions with others, and so have reasons to believe that they are due to a common remote cause.

Let us recapitulate the advantages that such an assumption offers us. (1) We have already seen that we practically never suppose that tactual perception deceives us as to the shape of the real. So much was this so that, except for the fact that cases might possibly arise in which we should have to flee to appearances of touch, we urged that there was no reason to deny the possibility that tactual objects of perception were real. If we think, however, that it is safer to believe that tactual objects, too, are appearances, they will have the advantage over visual appearances that, whereas the assumption that the former were all real would lead to terrible complications, no such complications would arise over the belief that tactual appearances were real. Now this is essential to the theory of a real spatial counterpart. To make this the counterpart of visually perceived spatial appearances would make the world of reality as complicated as it would be if we supposed that these visual appearances were themselves real. But to make it a counterpart of the tactual appearances will not lead to these complications because we already know that there is only one tactual appearance to an infinite number of corresponding visual ones. Hence the internal consistency and the comparative simplicity of tactual appearances make it reasonable to hold that, if there be a spatial counterpart at all, it will be to tactual and not to visual spatial appearances. (2) The assumption of such a counterpart avoids the assumption either that what we perceive is more than appearance, or that the reality that causes the perception is exactly like the object of the latter. It avoids the belief which we have already seen to be strictly impossible of proof, or even of conjecture, that the real remote causes of our perceptions have the same sort of sensuous particularity as do the objects of each man's perceptions for himself. But it replaces it by the belief that there is a one to one correspondence between perceived geometrical distinctions in the object of tactual perception and certain permanent ones in the reality events in which cause the perception. And this is all that we mean when we say that another man perceives the same object as we do. (3) It offers an explanation of the facts (*a*) that the shapes of visual appearances and of tactual ones are always correlated closely and may be identical, although the perceptions of which they form an analysable element must be very differently caused; and (*b*) that the tactual perceptions of different people agree, whilst their visual perceptions under such circumstances only differ slightly. (4) It thus enables the ordinary scientific theory of visual perception and its causes to be built up on the foundation of the essential reality of the shapes given in tactual perception. For it is to these shapes that there is a real one to one correspondence. The theory of a real counterpart to tactual appearances thus gains any probability that the success of the scientific theory of perception is able to reflect on its premises, and this is of course a very great deal. (5) On the other hand it is not tied down like the instrumental theory to accept every tactual perception that occurs as supplying true information about the spatial relations of the real. All the objects of tactual perception for it are appearances. To nearly all of them it is able to believe that there are real counterparts. On this assumption it builds up the scientific view of the world and in particular of visual perception into an immense self-consistent whole. If now it ever happens—as far as I am aware it *never* does—that a tactual perception occurs which leads to the belief in a 'real' shape which does not fit in with the rest of the scientific theory, and which cannot be explained away either as false judgment or merely partial perception, the present theory is prepared to meet it. It knew all along that *all* the objects of its tactual perceptions were appearances, but, on the assumption that to practically all of them there is a real counterpart, it has been able to build up a consistent theory of visual perception which has now attained to such probability from perpetual verification by experiment that in the case of these (possible) few objects of tactual perception it may be able to deny that there is a real counterpart to them on the ground that such a belief would be inconsistent with its whole well-verified body of knowledge.

Our conclusion, then, is that it is most probable that there is a real counterpart corresponding point for point to what is perceived in most (perhaps in all) the tactual perceptions that we have of figure, though doubtless more differentiated than the tactual objects themselves; and that events in this reality are the causes of our visual perceptions, according to laws which science, stating its position in terms of perceptible primaries, is able to discover. Whether there be any real correlate to the particular colours that we perceive over and above the real correlates of the imperceptible motions which science demands it is impossible to tell. That the real possesses something more than the correlate of tactual extension there is no reason to doubt; for tactual extension and visual extensions are both extensions of something. But what that something is and whether it or only events in it differ when we perceive different colours, temperatures, and 'feels' it is impossible to decide. Only this much can be said, that where, as in physical optics and in the scientific theories of sound and of heat (Kinetic Theory of Gases), we can make up successful hypothetical laws of the causation of our perceptions in terms of the counterparts of figures, spatial relations, and their changes alone; the belief that there are actual correlated *qualities* in the real is *pro tanto* weakened in probability. The reality of colours, and sounds, and temperatures, or rather of a real correlate to these general qualities in the remote causes of our perceiving them is not disproved by the scientific theory; these qualities are in the same depressing logical position as Dr McTaggart's non-omnipotent and non-creative God—'the only reason against their reality is that there is no reason for it.' In such good company we will leave them.

Before ending this rather tedious chapter we must, however, see how the causal theory has affected our previous position about dreams and drugs and instruments which we had to leave in a rather unsatisfactory state at the end of the third chapter. It will be remembered that in that chapter we came to the conclusion that, apart from the causal theory of perception, it was impossible to draw a distinction on any reasonable basis between the reality of the God whom the mystic can only perceive after certain definite initiatory processes, the pink rats that can only be seen by those who habitually take excess of alcohol, and the nuclei of cells which can only be seen by people who use microscopes after a certain prescribed method. We saw that the scientist wanted to put the cell-nuclei into an impregnable position, after which the Deity and the rodents must sigh in vain, and that there was grave doubt whether, even with the causal theory, he could do this. But we have travelled some distance since we left that chapter, and we may now be better able to deal with the question. The old difficulty was that it seemed monstrous to hold that the fact that mystics only experienced God, or people only saw everything yellow when they had performed certain rites or taken santonine, proved that what they saw was appearance, whilst the entirely comparable case of using a microscope did not prove any such thing. And our discussion of the instrumental view has only increased our certainty that any such arguments to prove that an object of perception is an appearance are invalid. The theory that the mystic initiation is a necessary means to the perception of a reality that cannot be perceived without it is *à priori* as reasonable a view as the alternative that those initiations are just the causes of the perception of appearances by the persons who employ them.

On the other hand, the upshot of the present chapter has been to show that, unless we want to have an immense complication of similar realities that are only perceptible from one position or by one person, we must in many cases reject the instrumental theory; and that when we once begin to do this in any case there are good grounds for doing so in all cases, certainly as far as sight is concerned. Hence we have now to explain why, when it is granted that what the mystic, the drunkard, and the microscopist perceive is equally appearance, we hold that the last mentioned gives additional knowledge about reality and are doubtful whether the two former do so.

At first sight the microscope seems to present a contradiction to the views that we have put forward as to it being reasonable to assume a real counterpart to tactual figure, but not in general to visual figure. For it almost seems as if we were here trusting our eyes as against touch for a knowledge of reality. A little consideration will prove that this is not the case and will justify the confidence that is placed in vision with a properly constructed optical instrument. If we assume that there exists as a rule a real counterpart to figures which we agree on perceiving by touch, we get the laws connecting these figures with what is perceived by sight under definite conditions as laws connecting the shapes of visual objects with the real counterparts of tactual ones. Thus are developed the theorems of geometrical optics. By applying these laws we learn that an arrangement of tangible points in a plane, if looked at through a microscope properly focussed and at right angles to that plane, will be replaced in visual perception by an object whose shape, at any rate at the centre of the field of vision, will not differ from the tangible shape, but whose size will seem greater than the tangible size. This last phrase means that the distance apart of two points seen through the microscope will be judged to be equal to a tangible length greater than that which we actually feel. Now it should be noticed that in this point, where there is a positive inconsistency between the teaching of sight as aided by the microscope and touch, we do *not* hold that sight is correct, but that touch is. That is, we believe that whatever corresponds to length in the real counterpart of the tactual figure agrees with it rather than with the lengths perceived by means of the microscope. But we also know that two points that may be perceived as one by the skin at one point may be perceived as two at other points. Hence it is fair to conclude that in the real counterpart when whatever corresponds to tactual distance diminishes beyond a certain amount it no longer is able to produce a tactual perception with two distinguishable points. Hence we are led to conclude that the inability to distinguish two points in our tactual perception does not prove that there is not a real distinction of the same kind as is responsible for the differences of tactual magnitude that we actually can perceive. Now we know from the general laws of light that a microscope makes two points which can be perceived tactually as distinct to be perceived as still further apart in the visual appearance, i.e. as being as far apart as two points which, seen by the naked eye, would correspond to a larger tactual distance. Hence it is reasonable to believe that in vision with a microscope really distinct points too 'near together' to produce any distinction of tactual character, are able to produce distinctions in the visual perception's object.

It is reasonable, then, to hold that in vision with a microscope we learn about real distinctions of the same type and in the same relations as those which do produce distinctions in objects of tactual perception and of visual perception with the naked eye, but which in their real relations have less of that magnitude which accounts for the perception of length than is necessary to cause perceptions with distinctions in their objects without the use of artificial aids to perception. In this sense, then, I think the belief that properly constructed optical instruments give us information about reality subject to limitations put upon them by the theory of light is justified. And, so far from conflicting with the belief in a real counterpart to tactual objects of perception it can only be properly defended on some such assumption.

We can therefore pass at once to the case of drugs and of drunkenness. Mach tells us in his *Analysis of Sensations* that if a person takes santonine everything will appear yellow to him. We may take this fact as a text for the subsequent discussion, assuming on Mach's authority that it is true. Most people would hold that the yellow colour, due to taking the drug, is a mere appearance, and even the scientist who thinks that all colours are appearances would hold that this was unreal in some special sense. *À priori* the following views are possible: (1) The taking of santonine is comparable with the use of a microscope. It causes the perception of an appearance with a distinction in it to which something in the reality corresponds, but which does not make itself felt in the perceptions produced in people who have not taken santonine. (2) The taking of santonine makes a real change in the real cause of the perception, so that it now causes a perception of a yellow object. (3) The drug affects the brain or other organs involved in perception and causes the same remote cause to produce a different effect.

I think we can practically dismiss the first alternative with a very few words. If santonine made us perceive a new colour there would be much to be said for the position here taken up. It might well be explained by the fact that the taking of the drug enabled us to have perceptions corresponding to wavelengths which now produce no effect on the mind. But it is quite clear that this is not what happens. We all agree with the person who has taken the drug in perceiving objects of practically the same shape and with some colour or other. Also we agree among ourselves about the colour and unite in disagreeing with the patient. Now the complete agreement, as we have already argued, suggests a common remote cause. But the difference is not one like we have between the microscope and the naked eye, that one perceives distinctions which the other simply fails to perceive. The drugged person simply differs from everyone else in the colour that he perceives; he does not perceive a fresh distinction; in fact, if Mach be taken to mean that he perceives everything as uniformly yellow, his position as against others is that of the colour-blind man against the rest of the world, rather than that of the microscope against the naked eye. If he be right then the agreement between other people to perceive various colours becomes inexplicable.

The second alternative cannot possibly stand by itself. If when *A* takes santonine changes do really take place in the remote cause of his perceptions which make it contain those events which cause the perception of yellow, you have to explain why none of the other people, all of whom are capable of perceiving that colour, fail to do so. Thus as an alternative to supposing that the drug affects *A*'s bodily organs of perception you propose a theory which involves that it affects those of everyone else. To offer such a complicated theory is ridiculous unless one is forced to it. Hence we are left with the usual conclusion that santonine merely affects the organs of perception of the man who takes it; so that the same remote cause which normally makes other people (and the patient when he has not taken the drug) perceive various colours, now makes the patient perceive all things as yellow.

It only remains now to notice the cases of delirium, dreams, and mystical visions. The case of delirium produced by excess of alcohol or by any cause which makes people have much the same perceptions, is not strictly comparable with that of santonine. For now the patients all perceive the same sort of things, as they did before, but these are no longer anything like what other people perceive. Now under these circumstances we cannot suppose that there is a common remote cause of the perception of the delirious and the normal observer, as we did in the case of the people who have and those who have not taken santonine, and who perceive much the same shapes. Hence this does leave open the possibility that the perceptions of the delirious man have real causes which are remote, but which fail to produce any perceptible effect on the minds of people who have not taken the drug in question. This possibility needs a little further discussion.

Of course the mere fact that even a delirious man's perceptions have an adequate cause somewhere is not of any interest in the present question; what we want to know is whether there is any ground for supposing that there exist remote causes for the existence of perceptions of pink rats by people who have drugged themselves with alcohol in the same sense as there exist remote causes for the perceptions of chairs and tables by people in their sober senses. Now I think that in the particular case of a man who sees pink rats with his eyes open there is strong ground for accepting the ordinary view that they are *mere* appearances in the sense that they have no remote cause which is in any sense a real counterpart of their shape. For pink rats are not things so very different from what ordinary people see every day. There is nothing peculiar about them except the fact that the rats that other people perceive are not of that colour. Moreover, they are localised at places in a room, where other people can neither see nor feel anything, by a man whose eyes are open. Now it seems to me that it is this close resemblance to the objects of ordinary perception and to the processes of ordinary perception ending in such a surprising discrepancy that is fatal to the belief in real remote causes that only affect the delirious through alcohol. The likeness to what can be perceived by normal people, and the open eyes, suggest that *if* there were such a remote cause it could not be—either in itself or in its actions—unlike the remote causes of our perceptions of chairs and tables and 'real' rats. But then it becomes inexplicable why it should only be able to produce perceptions in people who have habitually drunk to excess. Hence it seems clear that in this case the more normal explanation that these objects are produced entirely centrally, as an effect of the alcohol on the brain, is in all probability the right one.

When, however, we come to mystical visions in which people agree who have had the same sort of initiation, it seems to me we get further and further away from the case of santonine with which we started and nearer to a position in which the ordinary view of central excitation, though still possible, ceases to be so overwhelmingly the more probable explanation. Mystics often have their experiences when their bodily organs—at any rate the external ones—are not in use. Also they frequently say that they cannot describe the experiences that they have had in terms of the objects that they perceive in their normal life. Now it seems to me clear that the less their experiences resemble those of other people, and the less their way of getting them resembles ordinary perception, the less objection there is to the explanation that there is a special real remote cause of them in just the same sense as there is a real remote cause of those perceptions on which ordinary people agree. For it now ceases to be improbable that such real causes should exist and operate without producing perceptions by the usual channels in uninitiated persons.

I do not wish unduly to labour a point that would be more important for the philosophy of religion or of psychical research than for us, but as I hold that psychical research is a legitimate branch of natural science—though a difficult and backward one—I hope that I may be excused for adding a very few reflexions on this matter. There seem to me to be three possible cases to be dealt with if we assume the possibility of genuine veridical mystic vision. It might take the form of perception. In that case the veridical character of the vision would only mean that there really were common causes capable of producing perceptions of certain objects in mystics, but not in other people, and different from the causes of what anybody perceives in the ordinary course of nature. In that case we should be faced by precisely the same sort of problems for mystical vision as we found in ordinary perception, viz. whether anything, and if so what, can be determined about these common real causes from the objects of the perceptions which they cause. Or it might be held that in mystical vision we had true instrumentality, and the objects perceived were the real things. These questions it would be impossible to decide, even if we knew a great deal more about mystic vision than we do. For we have seen that they are far from easy, and can only receive more or less probable answers even for ordinary perception. Finally there is the possibility that what happens in certain mystical states is not a perception, but a feeling that a proposition is self-evident which is not present to other people. I find it difficult to understand what can be meant by, say, perceiving the doctrine of the Trinity. But I can understand that a man might in a vision become immediately certain of the truth of that doctrine, or that he might have a perception which he held could best be explained by believing that the doctrine was true. But we should have no ground for accepting either his immediate certainty or his explanation of his perception without examination just because he attained them in a vision. He might be, as many of us are in ordinary life, immediately certain of a proposition that is in all probability false; and he might, as we often do, propose as an explanation of his perception which was inadequate, or take as the only possible explanation one to which a more intelligent or experienced person could offer dozens of equally or more probable alternatives.

CHAPTER V

THE LAWS OF MECHANICS

We have now seen that there is some reason for believing and no reason for disbelieving that there exist real counterparts to the figures which all people agree in perceiving by touch. We have also seen that, when this assumption is made, it is possible to find laws between tactually and visually perceived figures and the visually perceived distances and positions of the latter relative to the body of the observer, and so to reason from a visually perceived figure of a given shape to the real counterpart of a tactual figure of definite shape events in which *do* cause the visual perception that we have, and which, if we performed the proper actions, *would* give us the corresponding tactual perception. We are thus justified in talking about geometrical relations, shapes, and distances as existing even when we do not perceive them, it being understood that we mean the real counterpart to which there would correspond a tactually perceived figure with the geometrical qualities in question if as a matter of fact we did have such a perception. Thus such a statement as: 'I cannot perceive an atom but I believe that atoms are shaped like dumb-bells' means that I believe that there exist in the real counterpart realities qualitatively like those, events in which cause visual perception of dumb-bellshaped figures, but the real quantity that corresponds to the sizes of lines and volumes and surfaces in perceived objects is so small that no perception is actually produced.

Now the laws of mechanics are laws that are found to enable us to connect the spatial positions of bodies at various times. In particular the so-called Laws of Motion are conditions which, it is believed, all material bodies obey, no matter what other information may be needed for a complete determination of their state. It follows that the laws of mechanics are well worth discussion. In the first place, if what has been said before be correct they are laws about reality or can become so by a simple change of statement like the one I have given above about the shape of an atom. In the second place, they give rise to much the most certain and important physical science that exists, and so they are good examples of causal laws at their best. In the third place, they raise a number of interesting questions, and their obscurity compared with the certainty of the structure that has been built upon them is yet another example of the truth mentioned by Mr Russell that in most sciences it is the middle parts that are simple and evident, and the foundations and the higher applications that are obscure and difficult.

I think that our best plan will be to state the three laws in a form similar to Newton's if not absolutely identical with it, and then to consider the notions involved in them and any problems that may arise with regard to them and the kind of evidence for them. Our discussions may of course necessitate a different statement at the end from what we had at the beginning.

The laws of motion as usually stated are (1) Every body persists in a state of rest or of uniform motion in a straight line unless acted upon by some external force. (2) The measure of the force acting upon a body at any moment in a given direction is the rate of change of momentum of the body in that direction at that moment. (3) For any pair of bodies *A* and *B* which exert forces on each other the forces are equal in magnitude, have the same line of action, and are opposite in sense.

The first law has been by some people supposed to be a barren tautology, and by others to be an *à priori* truth. But the most serious question that must necessarily precede any decision on these points is: What precisely is meant by the motion of a body in a straight line, and by its uniformity? Until you are clear about this you obviously cannot be sure whether that of which you are certain *à priori* is the first law of motion, because you do not know what is meant by the first law of motion.

Everyone knows that the paths in which bodies appear to travel depend, as we put it, on whether the observer is at rest or in motion on his own account. And the velocity which we attribute to the body will depend on the body relatively to which the distance travelled is measured and on the measurement of time adopted.

Hence, if you take all motion to be relative, you cannot talk about *the* direction or *the* velocity of a body, because no body has an unique direction or velocity. Each one will have dozens of different and equally good velocities and directions according to the bodies relatively to which its motion is determined. Similarly for the measurement of time. It is always measured by the recurrence of certain events, and, according as you take different processes to be uniform, you will reach different results as to the uniformity or lack of it in a given motion. Newton of course clearly recognised this. He distinguished between absolute space, time, and motion, and the relative spaces, times, and motions; and admitted that it was perfectly possible that no change was uniform and therefore capable of giving a measure of absolute time, and that no body was absolutely at rest or in uniform absolute motion in a straight line. But he thought that he could give a criterion of absolute motion, and, by experiment, prove the reality of at any rate absolute rotation. On the other hand, most philosophers have a strong objection to absolute motion, space, or time; and, putting this aside, there is felt to be a reasonable doubt whether it can be necessary or possible to start—as mechanics presumably does—from empirical data, to reach laws which are stated in terms of two entities which certainly cannot be perceived, and finally to return and explain empirically perceived motion by means of these laws.

Thus most of the recent discussions on mechanics have set the following problem before themselves: To state Newton's Laws of Motion compatibly with the belief that all motion is relative to material bodies.

Now we have seen that, if all motion be relative, you cannot talk about *the* motion of a body. But of course it might be possible to find one material system or a number of systems such that the motions of other bodies relatively to these obeyed Newton's laws. This has been attempted by Neumann, Streintz, and Lange.

Neumann's[1] theory is that there exists somewhere in space a body α, motions relative to which obey the

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[1] *Die Galilei-Newtonsche Theorie.*

Newtonian Laws. He compares the assumption of *a* to that of a luminiferous ether, but holds that it still more closely resembles the theory of electric fluids. It, like them, has a certain indeterminateness, since any other body *a'* that moved relatively to *a* with an uniform translational velocity and without rotation would do equally well for our fundamental body of reference. I do not agree with Mr Russell's[1] criticism of Neumann's body *a*. He admits that Neumann does not incur the fallacy of supposing that *a* is fixed (no small triumph for a relativist!). But he goes on to add that 'any statement as to its rest or motion... on his theory...would be wholly unmeaning. Nevertheless it seems evident that the question whether one body is at rest or in motion must have as good a meaning as the same question concerning any other body; and this seems sufficient to condemn Neumann's attempted escape from absolute motion.' This criticism seems to me to rest on a misunderstanding of those people who wish to keep both Newton's laws and relative motion. Relative motion without any further information is indeterminate. But it is perfectly possible that the relative motions of all bodies except one with respect to that one should be found to obey certain laws. It is not true to say that it is meaningless to make any statement about *a*'s rest or motion. This assumes that it is only those motions that obey Newton's laws that are real, which is tantamount to the assumption of absolute motion in a more rigid sense even than Newton's. For it is quite certain that *a* has motions relatively to other bodies, since other bodies have motions relatively to *a*; though *ex hypothesi* it will not, like other bodies, have one definite relative rest or motion to which Newton's laws will apply. *a* in fact, like all other bodies, will have a large number of relative motions at the same time; but, unlike other bodies, there will not be one of its relative motions that can be picked out as obeying Newton's laws.

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[1] *Principles of Mathematics*, Chap. LVIII. p. 491.

The trouble about Neumann's body *a* is that it is as incapable of being perceived directly as the points of absolute space. Hence it makes it no easier to see how the laws of Mechanics could have been discovered and justified by empirical observations. If *a* be essential to the laws of mechanics, then those laws must tacitly assert relations to *a*. But the laws of mechanics must have been induced from empirical observations of motions relative to bodies other than *a*. Now this must mean that regularities are discovered when we refer motions to some bodies *A*, *B*, *C* ... etc. Hence the observed regularities are regularities in motions relative to *A* or *B* or *C* .... Thus the argument for *a* will have to be as follows: If you replace the motions relative to *A* or *B* or *C* by motions relative to a body *a* and suppose that these relative motions obey the Newtonian laws you will be able to explain the observed regularities in motions relative to *A* and *B* and *C* .... But I fail to see that this furnishes any evidence for the existence of *a*; all that we know is that, relative to it, we can ascribe such velocities in such directions to observable bodies that, when subjected to the Newtonian laws, they will account for the observable relative motions of the bodies. But we must note that, when we have assumed the Newtonian laws and the body *a*, we have still to fix the Newtonian masses and forces and the particular velocities and directions of the assumed motions relative to *a*, so that the results shall agree with the observed relative motions. Can we suppose that agreement with observed regularities will suffice to determine all these variables, and to prove the truth of the laws, and to prove the existence of *a*? The fact is that if the Newtonian laws be true of some relative motion there must be regularities of some kind amongst motions relative to any body in the universe that you choose to take, and, since all relative motions are equally real, the various laws thus reached will be merely mathematical transformations of each other. The sole advantage of the Newtonian system with a body *a* will be that the laws will be much simpler. But this is not the slightest ground for believing in the existence of the body *a*. To do so on such grounds would be strictly comparable to holding that, because the equation of a curve becomes simpler by suitable changes of axes and shifting of origin, there is reason to believe that the particular position of the origin and the particular inclinations of the axes that give the simplest equation are real in a sense in which other axes and origins are not. Thus I conclude that Neumann is quite wrong in comparing the evidence for the reality of *a* to that for the reality of the ether. He is no doubt nearer the mark when he says that it is more comparable to the electric fluids; for the electric fluids are merely mathematical fictions, which however have a slightly better claim to be considered as possible realities than *a*.

In fact we can come to a general conclusion about all efforts to state the Newtonian laws in terms of relative motion. Either they mean that, if we take all motions relatively to axes defined by certain perceptible bodies like 'fixed' stars, Newton's laws are approximately obeyed. Or they mean that we can so replace the perceived relative motions by motions relative to an imaginary body that, if we assume that these motions obey the Newtonian laws, we can account for all the observed relative motions. This, however, will not give the smallest reason for supposing that the imagined body is real or that the Newtonian laws are truer than others that would result if the motions were taken relatively to some other body.' It will merely tell us that, in so far as the results of this hypothesis agree with the observed relative motions, there will be some laws connecting the motions of bodies relative to *any* body that we choose to take, which will be compatible with the Newtonian laws and will be merely mathematical transformations of them, but will be much more complex. The Newtonian laws and the imagined body will be comparable, not to a fruitful scientific hypothesis which is constantly verified by experience; but to a happy choice of axes and origin which renders otherwise intractable problems soluble. That would not of course be any disparagement to the Newtonian laws nor to the genius of their discoverer. It would still have been one of the most important discoveries ever made that such a transformation can be found. But it would be more than this. For, in connecting all the observed regularities in the motions of bodies relative to other bodies, it would have shown that there are general laws connecting *all* motions, no matter to what body they are taken as relative. Otherwise it would be perfectly possible that there should be motions that obey no laws; and this would also involve the proposition that the motions of *all* bodies relatively to some bodies were without regularity.

But, it may be said, if *a* be purely imaginary, what is the use of calling it a body? Have not the axes in *a* become indistinguishable from the supposed axes in absolute space? Moreover, as Mr Russell asks, if the axes be material how can you deny that they may be subject to forces? And, if they be subject to forces, they will be accelerated and therefore will not be axes relative to which Newton's laws will hold. Mr Russell remarks that the question whether all bodies be subject to forces must be at least significant, and that this is fatal to the relative view. Now I do not think that this is a fair argument against the theory in question, because it seems to me to rest on the assumption of the opposite view. If Newton's laws be really laid down for motions relative to absolute space then it is reasonable to hold that they must be true or false whatever bodies we may be considering, and that it must be always significant to ask this question. But suppose we were to put the laws in the form that they hold as far as we can gather, for the motions of all observable bodies relative to axes defined by the fixed stars. It would follow from this that if the laws hold rigidly of the motions of other bodies the fixed stars *cannot* be acted on by forces. What would the statement mean then, which we should be much more likely to admit, that the fixed stars too are very probably acted on by forces? If we are going to use 'forces' in the same sense as before and in the only one that Mr Russell could admit, it would mean: Very probably the fixed stars do move with accelerated motions relative to ax~~i~~es for motions relative to which the Newtonian laws hold. Now the only axes for motions relative to which we have any reason to suppose the Newtonian laws hold are defined by the fixed stars themselves. And it is quite certain that the stars cannot be accelerated relative to axes defined by themselves. Hence the meaning of the phrase would be: Very likely there are bodies such that, if axes be defined by them, the fixed stars have accelerations relatively to those axes which obey Newton's laws. This would of course assure us that our original belief that motions of observable bodies relative to axes defined by the fixed stars obey the Newtonian laws cannot be strictly accurate. Hence we can always give a meaning to Mr Russell's question on a purely relative theory. His question is: 'Are your bodies of reference β_1 subject to Newtonian forces or not?' We answer (1) that this question, on the relative theory, must mean: Are there any bodies of reference β_2 relative to which your bodies of reference β_1 have accelerations that obey Newton's laws? And (2) the answer to this question is (*a*) that, if you hold that the observable bodies X which you said obeyed Newton's laws for their motions relative to β_1 really obey them accurately, then there cannot be a set of bodies β_2 relative to which the bodies β_1 have accelerations that obey Newton's laws. In that case the bodies β_1 are not subject to Newtonian forces. But (*b*) if, on the other hand, you admit—as of course you must do—that the motions of the other bodies X relative to axes defined by β_1 can only be known to obey Newton's laws within the limits of experimental error, then it is perfectly possible that there may be a set of bodies β_2 relative to which both the bodies β_1 and the bodies X are accelerated according to Newtonian laws, but that the nature of their respective motions is such that the motions of X relative to β_1 also approximately obey Newton's laws. In this case the bodies β_1 may be subject to Newtonian forces. Of course the same sort of question may be asked of β_2 and the same sort of answer can be given and so on *ad indefinitum*. But I do not see that there is anything objectionable in such a regress; it certainly makes no difference to mechanics as an empirical science, and Mr Russell's question can be made significant on the relative theory of any set of bodies of which he chooses to ask it.

There is one further point to notice before leaving this subject. The relativist is not obliged in fact, and least of all on Mr Russell's theories of causation in general and in mechanics, to make his axes real bodies. What he wants is to find such axes as will give the laws of the actual relative motions of bodies in the world in the simplest possible form. If he can simplify his laws by taking to define his axes bodies which he merely imagines in places where no bodies actually are and by ascribing such relative motions to them with respect to the real bodies in the universe as are compatible with the relative motions of the latter, he is at perfect liberty to do so. For instance, it would be perfectly legitimate, after we have found that the first law of motion is approximately obeyed for motions relative to the fixed stars and have gone on to discover that the fixed stars are actually in relative motion to each other, to ask ourselves whether we cannot, by imagining bodies in places where they cannot be observed and ascribing to them certain motions relative to the real bodies in the universe which are compatible with the latter's actual motions relative to each other, get a set of axes such that the motions which we must ascribe to real bodies relative to them accurately obey the First Law. There are only two plausible objections that could be brought against this. (1) It might be said that the accelerations relative to imaginary bodies are imaginary accelerations and therefore cannot be the effects of real causes nor the causes of real effects. But, as Mr Russell well shows, on *any* view of mechanics this difficulty, if it be a difficulty, will arise over the resolution and composition of forces; so it is no special objection to the relativist theory. But actually when we have banished the view that causation involves activity and have grasped the fact that it means nothing but laws, the difficulty vanishes. If the laws can be more simply stated in terms of motions that do not really take place than in terms of any that do there is no objection to using the simpler form when you know that you can always get back to the actual motions when you wish. The imaginary bodies and their imaginary motions are to be regarded as parameters, and the fact that they do not exist in nature is no more reason for refusing to state laws of nature in terms of them than is the fact that the cycloidal arches of Westminster Bridge were not actually made by rolling circles on straight lines a reason why an engineer should not simplify his calculations about them by imagining them so produced and introducing parameters accordingly. (2) It might be objected that if you introduce imaginary bodies and motions you might as well introduce absolute space. To this the answer is simple. If you only mean that as a mathematical device the statement of laws in terms of absolute space is as good as the statement in terms of imaginary bodies and motions we may agree in the main. But you must not suppose that this proves the reality of absolute space any more than it proves that of the imagined bodies and motions. And the relativist might reasonably add that his plan involves a less violent effort of imagination, since we are acquainted directly with correlates to real bodies and real relative motions, whilst we have no direct experience of anything corresponding to real points and real absolute motions.

But of course with these objections removed the theory of the relativity of motion is still by no means out of the wood. There are first of all the supposed experimental arguments for absolute rotation which have to be considered; and then there is an argument for the necessity of absolute translation as being what we really mean by motion to be discussed; and finally we must ask by what positive arguments the theory of the relativity of motion proposes to support its position.

We will begin with Newton's bucket, and the paradoxes that have been held to spring from a denial of absolute translation. One very startling paradox is mentioned by Neumann[1]. He points out that if we take the case of a material point revolving round another under their mutual attraction and imagine all the other bodies in the universe annihilated it would follow that the two points must run into each other along the straight line determined by them at the moment at which this cosmic disaster happened. For, on the relative theory, all motions must be changes of distance between points or of angles determined by at least three points. Now the last possibility vanishes because there are no longer three points in the universe. Hence the result follows that the two particles will now run into each other according to the ordinary law of attraction. I confess I do not see why the relative theory should make difficulties for itself by talking of 'material points.' This particular trouble would not arise in so acute a form if you considered two finite bodies like the sun and the earth; for you could define a plane in the sun by taking three marks on it and then observe the change of angle between it and the line joining one of the spots to the centre of the earth. But of course this is a part of the general difficulty about any relative theory. If it makes its points finite and perceptible you cannot talk of *the* distance and *the* plane that they define. And, if you make them material points, then, though you have avoided this difficulty, your points have ceased to be perceptible and it is difficult to see what advantage you now have over the absolute theory of space. But this is a question for geometry rather than for mechanics. Still we shall see that it is impossible to state the third law or to apply the laws of motion to rotating bodies without introducing particles, so that Neumann's paradox is a well-founded one.

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[1] *Op. cit.* p. 28.

But the more celebrated argument for absolute rotation is Newton's bucket experiment. Everyone knows what this is. A bucket with water in it is rotated about its axis. (1) The bucket rotates relatively to the water and the surface of the latter remains level. (2) The water finally takes up the rotation of the bucket. There is then no relative motion between the bucket and the water, but the surface of the latter becomes depressed in the middle and assumes a paraboloidal form. (3) The bucket is now stopped and the water goes on rotating. It is now rotating relatively to the bucket and it still retains the paraboloidal form. (4) Finally, the water ceases to rotate relatively to the bucket, and the surface is once more flat. This experiment, Newton held, distinguished between absolute and relative rotation and gave a measure of the former. We will discuss these two arguments in turn.

To begin with Neumann's. If Neumann's assumption that all the bodies in the universe be annihilated be taken literally it is clear that it must include the annihilation of the body *a*. For the two particles that are left cannot possibly be the body *a*, since that must consist of at least four particles. But, since Neumann holds that the laws of mechanics can only be stated in terms of motions relative to *a* he goes too far in saying that the particles would run together along the straight line joining them at the moment according to the law of gravitation. For to know how they would run together involves an integration based on the assumption that the first law of motion still holds, and Neumann has been insisting that that law is meaningless until we introduce *a*. But we may agree that the particles must keep to this straight line however they may move in it. What one does not quite see is what relevance the whole example has to Neumann's particular theory. For Neumann's theory is a particular account of the statement of Newton's laws in terms of relative motion. This particular paradox might be valid as a ground for rejecting relative motion altogether, but it cannot be any ground for accepting Neumann's particular theory of the body *a*; all that could result from it would be that the most terrible consequences would follow the annihilation of the body *a* when we are given the relativity of all motion and *a*'s peculiar position among bodies.

But the question is: Does this paradox, which certainly proves nothing in favour of Neumann's theory, prove something in favour of the theory of absolute rotation which contradicts Neumann's? Mach[1] holds that it does not. He accuses Neumann of 'making a meaningless assumption for the purpose of eliminating a contradiction,' and of 'making too free a use of intellectual experiment.' I suppose that the 'contradiction' is to be found in the two propositions: (1) The existence of other bodies not exerting forces cannot be relevant to the motion of a body, and (2) that, if the relative theory of motion be true, the annihilation of all other bodies does change the motion of the remaining two. The 'meaningless assumption' must be the denial of the relative theory. It is impossible, however, to agree that there is any *contradiction* to be removed, on the relative theory; and I do not see that either Neumann or Mach (who accept the relative theory) can possibly hold that there is. It is merely forgotten as usual that, on the relative theory, every body has at the same time a number of different motions in different directions, according to the body to which the motion is taken as relative. When any bodies are taken to cease to exist the motions relative to these naturally cease to exist also. But the other proposition which asserts that the disappearance of bodies not exerting forces is irrelevant means that it is irrelevant to motions relative to a Newtonian frame of reference. If the bodies that constitute this disappear too then naturally the Newtonian laws cease to apply, since the motions to which alone they apply have ceased to exist. But there is no contradiction in this to the other statement. There is merely a paradox because unsophisticated persons do not believe that motion is purely relative to other bodies. Thus the whole question is whether this belief which makes Neumann's result a paradox does or does not rest on a 'meaningless assumption.' To assert that it does is to assert without proof that what nearly everyone believes is not only false but meaningless.

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[1] *Principles of Mechanics*, Eng. Trans. p. 572. [He is discussing another argument of Neumann's, but it is quite parallel to the present one.]

The other criticism of Mach's is worthy of more serious consideration. Apparently Mach's objection is that this result rests on too violent an abstraction. He says: 'When experimenting in thought it is permissible to modify *unimportant* circumstances in order to bring out new features in a given case; but it is not to be antecedently assumed that the universe is without influence on the phenomenon in question[1].' This is clearly very unsatisfactory. 'Unimportant' is purely subjective in this reference; apparently things become important when the abstraction of them lands Mach's theories in paradoxes. But then their importance is relative to Mach rather than to the universe at large. In fact the whole argument is irrelevant. Either motion is relative, as Mach believes, or it is not. If so then all bodies in the universe are equally relevant and Neumann's result follows strictly. If not[2], we may certainly agree with Mach that the laws of mechanics which have been induced from motions that took place in presence of the rest of the material universe would very likely not hold when there were only two bodies in it. But that is not the least ground for supposing that the laws that hold under these circumstances would be such that the bodies would move in absolute space in the same way as they would on the relative theory when there were only two bodies left. If absolute motion be a 'meaningless assumption' this would be a meaningless suggestion; and if not, there is no reason why the laws of absolute motion when there are only two bodies in the universe should agree with those of relative motion under like circumstances, when it is notorious that, for many relative motions, they do not when there are more bodies. Mach's arguments in fact are both logically fallacious, one is a *petitio principii* and the other an *ignoratio elenchi*. The whole paradox arises out of the fact that we instinctively believe that motion is not purely relative, and the whole question is whether this belief in any form can be justified or refuted.

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[1] *Loc. cit.* (the italics are Mach's).

[2] I take the object of Mach's argument to be to show that Neumann's paradox is not really so very paradoxical, because, even if we assume absolute motion, we have no right to assume that, when the universe is reduced to two bodies, the laws induced from such a widely different universe as ours would apply. It is on this view of his meaning that I discuss the passage.

Before we discuss this question, however, we must consider Newton's bucket. This celebrated experiment, together with gyroscopes and Foucault's pendulum, has been the great argument used by believers in absolute rotation, among whom we may mention Newton, Kant (in a peculiar and characteristic modification), Maxwell, Streintz, and Mr Russell. What does the experiment immediately prove? It proves that the relative rotation of the water and the bucket is irrelevant to the shape of the surface of the water. The water is flat when both bucket and water are at rest relative to the earth, and paraboloidal when both are moving with the same velocity relatively to it. Hence both states are compatible with relative rest of the bucket and the water. On the other hand both are also compatible with relative motion of the two. The point that is relevant to the shape of the surface is the motion of the water relative to something other than the bucket, I must confess that this seems to me to be perfectly compatible with the relative theory. The water might well become curved when it moved relatively to the fixed stars, but have its shape quite indifferent to its motion relative to the bucket. So far I agree with Mach. But he then proceeds to add the statement that: 'No one is competent to say how the experiment would turn out if the sides of the vessel were increased in thickness and mass till they were ultimately several leagues thick[1].' The precise relevance of this remark I have never, after the longest consideration, been able to fathom. So far as I can see Mach's position would be that we can account for the motions that are perceived by Newton's laws if, wherever Newton uses absolute space, velocity, or acceleration, we substitute position, velocity, or acceleration relative to axes defined by the fixed stars. This is at least a plausible position; but, if it is to be of any use, it must enable us to give at least a probable account of what would happen if the walls of the bucket became several leagues thick. If Mach merely wishes to tell us that we cannot be perfectly certain what will happen under circumstances that we cannot observe this is no doubt quite true, but it is surely rather platitudinous. But how the proposition that we cannot be certain of what will happen under a given set of circumstances can increase the probability of any proposition except that we live in an uncertain world is more than I can understand.

I conclude then that, whilst Newton's bucket might possibly be a test between absolute and relative rotation for those people who already believe in absolute rotation, there is nothing in it that is incompatible with a purely relative theory of motion, and therefore it cannot decide between the two rival theories.

We pass then to the Foucault pendulum which is supposed to prove to us that the earth rotates absolutely. Let us once more begin by considering what it is precisely that we observe in the Foucault experiment. We swing a large pendulum and we note the plane in which it begins to swing[1]. We will suppose that we make a chalk-mark on the floor in the line where this plane cuts it. As time goes on, if we draw lines on the same principle we shall find that they make angles with each other. From a consideration of these angles and the lapse of time we can calculate the velocity of the earth about its axis; and we find that this value agrees with that reached from observations made on the fixed stars. It is surely perfectly plain that what we *observe* is not absolute rotation, but the change of position of a plane with respect to its former position as marked out by material lines. Hence the conclusion that the earth rotates about its axis with a certain absolute angular velocity must be deduced from these observed premises by reasoning. The results of that reasoning must contain a reference to absolute time and space which was clearly not contained in the premises which were observed. But new entities cannot get into a conclusion unless they were introduced somewhere in the course of the reasoning as supplementary assumptions. As a matter of fact they were of course introduced in the laws of motion and in particular in the first law. For it was on the first law and on that alone that it was decided that the plane in which the pendulum swings is fixed in absolute space. Thus the Foucault pendulum only proves absolute rotation if you have already assumed laws of motion in terms of absolute space and time. By itself it merely exhibits a particular case of relative motion.

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[1] *Principles of Mechanics*, p. 232.

But, it will be asked, does not the fact that the angular velocity deduced from this experiment agrees with that reached from the observations of fixed stars prove anything? Certainly it proves *something*, but not, so far as I can see, anything about absolute space and motion. What it proves is that the plane in which the pendulum swings is, within the limits of experimental error, at rest with respect to axes determined by the fixed stars. This tells us that, if the sky had always been covered with clouds, we might have reached Newton's laws if we had referred all motions to systems of coordinates defined by the planes of Foucault's pendulums and the perpendiculars to them. This seems to be the best that can be said for Streintz's[1] position that we must define what he calls Fundamental Bodies and Fundamental Systems by their non-rotation as proved by gyroscopes and pendulums and then enunciate the Newtonian laws with respect to them. But Streintz ruins his position by coming to a statesmanlike compromise (worthy of the best traditions of English public life) and accepting absolute rotation, whilst he holds that absolute translation is meaningless. This is a double error. In the first place we have seen that such experiments do not prove absolute rest or motion apart from the assumption of Newton's laws in terms of absolute space and motion. In the second place these laws are stated in terms of absolute *translation*, which Streintz says is meaningless. And, finally, if you accept absolute rotation you have accepted absolute direction at any rate, and, although this does not positively necessitate absolute space, it does not seem to be an easier assumption or a more likely one.

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[1] I shall assume that the pendulum is swinging at the N. Pole. This makes no difference of principle and simplifies the statement.

I hope that I have now shown conclusively that it is circular to pretend that such experiments *prove* absolute rotation; they merely give a ground for deciding between absolute and relative motions to those people who have already accepted absolute motion and formulated the laws of mechanics in terms of it.

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[1] *Die physikalischen Grundlagen der Mechanik*, pp. 24 and 25.

Mach uses his well-known argument against the Foucault pendulum experiment that it cannot be said to prove that the earth would rotate even if there were no fixed stars. He says that: 'The universe is not *twice* given with an earth at rest and an earth in motion; but only once with its *relative* motions alone determinable. It is accordingly not permitted to us to say how things would be if the earth did not rotate[1].' Mr Russell[2] severely criticises this argument, but I cannot accept his criticism as it stands. After repeating the first of the two sentences that I have quoted from Mach, Mr Russell adds: 'Hence any argument that the rotation of the earth could be inferred *if* there were no heavenly bodies is futile.' I do not know if Mach drew this conclusion in his first edition from which Mr Russell quotes. As will be seen in the quotation which I give his conclusion is different in the English translation. His argument seems to me to be the perfectly sound one that the earth is never *given* (i.e. in experience) as absolutely at rest or in motion, and that he does not see how we can pass from the observed relative rest or motion to a supposed absolute rest or motion divided in certain proportions between the two terms. The conclusion that Mr Russell gives and that I am unable to find in my edition of Mach is obviously foolish on Mach's own principles. It is in fact ambiguous. It might mean either that if there were no heavenly bodies there might be nothing relatively to which the earth could rotate, or else that, if the heavenly bodies ceased to exist, the earth might cease to rotate relatively to something that remains observable whether there be heavenly bodies or not, and relatively to which it did rotate when there were heavenly bodies. The first alternative is disproved by the Foucault experiment, since that does give an observable relative rotation whether the fixed stars can be observed or not. But if Mach, as interpreted by Mr Russell, merely means that, if there were no heavenly bodies, the Foucault experiment might cease to give the same result, I really do not see how we could disprove this, or how it is relevant to the question of absolute or relative motion. It is in fact a repetition of Mach's argument against Neumann which we have already seen to be intrinsically valid but a platitude, and, in the use to which it is put, an *ignoratio elenchi*.

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[1] *Principles of Mechanics*, p. 232. (Italics are Mach's.)

[2] *Principles of Mathematics*, p. 492.

I must confess that I do not understand Mr Russell's condemnation of the argument in itself. For he condemns it, not because he thinks that its present use is an *ignoratio elenchi*, but because he holds that it rests on an entirely false view of the nature of mechanics as a science. He says: 'The logical basis of the argument is that all propositions are essentially concerned with actual existents and not with entities which may or may not exist. For if...the whole dynamical world with its laws can be considered without reference to existence then it can be no part of the *meaning* of those laws to assert that the matter to which they apply exists...[1].' This seems to me a very curious argument. I can only meet it by asking whether it be or be not true that mechanics is a science the laws of which are based on empirical observations. Mr Russell at least is hardly likely to deny this. Now those empirical observations have all been made in the presence of the heavenly bodies. Also all laws based on observation are only probable, and it is generally and reasonably held that their probability diminishes as the state of affairs to which it is proposed to apply them departs more and more from that under which they were originally observed to be obeyed. Hence it seems to me that Mach is perfectly correct in holding that it is quite possible that the Foucault experiment might not succeed if all the heavenly bodies were annihilated. Such a conclusion, as we have seen, is not relevant to what he is trying to prove, but it seems to me a valid conclusion as against Mr Russell's criticisms. It does not rest at all on the logical basis that 'all propositions are essentially concerned with existents' but on the proposition that all general laws that rest on empirical evidence must depend for their truth on observations of the existent, that they can never be more than probable, and that their probability diminishes the further the state of affairs under consideration differs from that from observations of which they were originally induced. Nor does this conclusion conflict in the slightest degree with the fact that rational dynamics makes no mention of the fixed stars, or that the problems with which works on rational dynamics are full have no reference to real distributions of matter. For what is Rational Dynamics? It is the mathematical results that follow from the assumption of Newton's laws. True, Newton's laws, on the relative theory, are those that are found to hold of the existent world for those motions that take place relatively to axes defined by the fixed stars.

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[1] *Principles of Mathematics*, p. 493.

But this does not prevent rational dynamics from dropping the fixed stars, replacing them by axes in an assumed absolute space, and supposing that all motions relative to such axes obey the same laws as those that are observed to hold in the real world for motions relative to the fixed stars, and working out the results of these assumptions for any assigned imaginary distribution of masses. You might perfectly well make up a non-Newtonian rational dynamics, and the only difference would be that its results would not be applicable to the motions of real bodies relative to the fixed stars. In fact we may end this discussion by proposing the following dilemma to Mr Russell. Either Newtonian dynamics is purely rational or not. If so I fail to see how it can throw any light on the existent and how Mr Russell can be justified in such statements as that Foucault's pendulum and Newton's bucket prove the existence of absolute space and motion. But if not, then, though no appeal to the existent is needed in proving its conclusions from its premises, you cannot be sure that the conclusions would apply to an existent so different from that from which the premises were originally induced as a universe that consisted of nothing but the earth with all the heavenly bodies annihilated would be from the total universe that we know and observe.

I agree then with Mach that none of the arguments so far produced are incompatible with a purely relative theory of motion, so long as the theory understands itself.

Experimental arguments from the motions of matter then will not help us, and we must pass to other ones. Let us begin by asking on what grounds precisely relativists deny absolute space and motion. I think there are three. The first is that they cannot be perceived and that we can do without them. The second is that they are meaningless. And the third is that space involves contradictions. Arguments like the second are always cheap and easy and rather contemptible. Taken literally the argument asks us to believe that the *words* 'absolute space' and 'absolute motion' are *mere* noises or marks on paper which convey nothing to anybody. The best way to refute this opinion is actually to state what the difference between the two views means, and this I do lower down. In the meanwhile it is only necessary to note (what many people forget) that to say that absolute motion is meaningless *and* to produce further arguments against it is inconsistent. You cannot refute a noise or overthrow a mark by a syllogism; you must confine yourself to saying that you do not understand what Newton and Euler and other supporters of absolute motion were talking about, and that you strongly suspect that they themselves did not know what they were talking about. And you will run some risk of having the first statement believed and the second doubted. The third point need not detain us either. The arguments to prove that space cannot be real because, if it were, contradictory propositions would be true about it, all rest on sheer errors about infinity and continuity. For their refutation we have merely to refer to the relevant chapters in Mr Russell's *Principles of Mathematics*.

The sole argument then that is left to the relativists against the belief of common-sense in absolute motion is that it is not perceptible. Is this any argument at all? It will be remembered that we have tried to show in the earlier parts of the present essay that in all probability nothing that is perceptible is real. Hence we should be more disposed to believe that absolute motion is an appearance if we could perceive it than to believe that it cannot be real because it is imperceptible. We must therefore try to show what is meant by the distinction between absolute and relative motion on our view. We have expressed a belief in the existence of a real counterpart to those spatial relations which we agree in perceiving by the sense of touch and to those objects of visual perception which science, based on the belief of a real counterpart to tactual space, is able to justify. But what is perceived by sight or touch always has other qualities beside spatial ones. The spatial qualities that we perceive involve relations, and those relations must have terms. Those terms we took to be real but unknown qualities in real counterpart relations, and we supposed that the changes in these caused our perceptions of colour and temperature and also of perceived spatial relations. Now the question of absolute space for us simply is this: Do the real correlates to perceived spatial relations hold directly between these unknown terms or is there a *continuum* of other terms between which real relations eternally hold, so that the correlates of perceived and changeable spatial relations are really complex and arise from the terms between which they hold being 'at' now one and now another of this *continuum* of terms which stand in eternal relations to each other? The latter supposition is what would correspond to absolute space on our theory. We may at once refer to these supposed terms as 'geometrical points,' since it is admitted that such points could not be perceived. We can at least see that the question at issue is not meaningless. We cannot perceive either points, if there be such things, or those other terms between which we believe the relations correlated to the felt spatial relations hold. But, whilst points cause no perception in us at all we believe that the other terms or events in them are the causes of perceptions. We will call the latter set of terms 'material qualities.' The question then can be put as follows: Granted that there are material qualities in certain real relations closely correlated with those spatial relations that we perceive by touch between sensible things, and granted that events in these material qualities cause our perceptions of pieces of matter of various shapes and sizes and in various motions, are we to say that these real correlated relations are simple and variable relations between such material qualities? The affirmative answer to this question is the relative theory. The other alternative is that there is a real *continuum* of geometrical points which eternally stand to each other in certain relations and do not produce any perception in us. The real correlated relations to the spatial relations that we perceive will then not be simple but will arise out of a certain relation of material qualities to variable geometrical points + the eternal relations of these points to each other. This is the absolute theory in terms of our view of perception and of the probable nature of the real. Since, on our theory, we cannot perceive either geometrical points or material qualities, the fact that geometrical points are imperceptible is very much less important than it might be on some other theories.

It will be said, however, that the admission that, unlike material qualities, geometrical points and their eternal relations produce no perceptions in us, takes away all reason for believing in them. But this is not strictly true. For common-sense holds that not all motion is relative, and, if it be correct in that opinion, something like geometrical points will be needed to make our theory correspond to the views which common-sense holds and can justify.

The question then is: Can common-sense justify this belief? Is it ultimate, or does it depend on bad reasoning or on psychological causes that are competent to produce the belief but are equally or only compatible with the truth of the relative theory? If the belief were ultimate it would, I think, be very unwise not to give due weight to it, since we have now seen that there is absolutely no argument against its possibility. For the question turns on what is meant by motion; and that, after all, must ultimately rest on what people find, after careful reflexion on all the possibilities, that they actually do mean by it. If, after looking at all the alternatives, people still think that it is essential to what they mean by motion to be able to say of two bodies that move relatively to each other that the real motion of one is so much and that of the other so much, or that one really moves and the other really stands still, it would be unwise to accept the relative theory which makes such statements falsehoods.

Now it is not to be denied that, if bodies have absolute motions at all, people constantly make mistakes as to what they are. We feel instinctively that the earth stands still and that motions relative to it are absolute motions. But the fact that people often or always make mistakes in assigning the magnitude of a quality q in given cases, does not prove that there is no such quality as q, or that it is not an essential part of the notion of that process in a particular case of which it was wrongly assigned. The very fact that people always assign it somewhere and in some degree in the process shows that they at least believe that the quality in question is in general essentially bound up with the meaning of the process. It might very well be possible to explain their mistakes by psychological causes without being able to explain away that characteristic about which their judgments were mistaken.

In the first place, it is clear that in many cases of perceived motion something is perceived in one body which cannot be observed in the others. Thus, if I see someone walking up the Hall there is a positive perceptible difference from what I should see if the end of the Hall moved toward him, although, on the relative theory there can be no difference as far as motion is concerned, since that merely consists in the shortening of the distance between the man and the wall. Thus, it seems necessary to hold that, even if we cannot perceive absolute motion, we can perceive distinctions in the case of two bodies that move relatively to each other which are interpreted in terms of absolute motion. This, however, need not trouble the relativist. For, in the case under discussion there are differences of relative motion that account for the difference perceived. The man who is said to be 'really' moving really is moving relatively to my body, whilst the wall at the end of the Hall is not. Hence, while it might remain true that all motion is relative, still the man who is said by common-sense to move will indeed have a relative motion which the end of the Hall, which common-sense calls stationary, will not have. Again, we are quite sure, with respect to our own bodies, whether we move or something else moves relatively to us; but then the actual perceptible difference is that, whilst in both cases we perceive the same relative motion between our own body and the other one, in the one case we also have certain muscular sensations and perceive that we are also moving relatively to other bodies, and in the other we have no muscular sensations and no perception of relative motion between our own body and other bodies than the given one, whilst the given one is now perceived to be in relative motion to others beside our own.

I do not think that there can be any doubt that there is a peculiar perception both tactual and visual which we can call perception of motion. And motion so perceived is undoubtedly absolute, in the sense that the direct object of the perception is not the mere change of a relation between two bodies but is ascribed as a quality to one or both of them. But all this is in the realm of appearances. When you come to ask what it is in reality that causes the perception of such appearances you can always, it seems to me, explain them just as well in terms of real relative motion as in terms of real absolute motion. The belief that motion is absolute, which common-sense holds, seems to spring from a confusion between these two different senses of motion; viz. motion as the object of a perception and therefore an appearance, and real motion. Apparent, i.e. perceptible, motion is always absolute in the sense described above. It is always ascribed as a quality to the moving body or bodies. But our previous investigations have led us to believe that all such perceptible qualities are appearances. And, at any rate, the motion that is thus absolute is not that which mechanics takes to be absolute. The perceptible absolute motion of the earth is zero, whilst the perceptible motion of telegraph-posts from a train window though quite as absolute as any other perceptible motion is not that which mechanics has to ascribe to them.

But it is not unnatural that the absoluteness which attaches to the apparent motions which common-sense perceives should be carried over into the account that science gives of the real causes of the perceptions of these appearances; and so it is supposed that motion must be absolute in the widely different meaning that that term has in mechanics. Now the theory of absolute motion is indeed a possible explanation of these appearances, though, in as far as it is admitted that the appearances often cause us to make mistakes about the real absolute motion, it can hardly be called a completely satisfactory one. But it is also possible, as we have seen, to explain these distinctions in the appearances in terms of purely relative motion + muscular sensation. We must remember that, even if *A* and *B* could only move relatively to each other, so that there were no sense in saying that it was really *A* that moved whilst *B* stood still, yet there might still be reason to believe that the cause of the change of their mutual distance was in one of the terms only, and this would make a distinction between the two which, owing to the prejudice carried over from apparent absolute motion, would come to be held as a mark of real absolute motion. And I do not think that the phenomena of motion offer us any means of deciding between the two possible alternatives.

Thus, my conclusion is that common-sense believes in real absolute motion because it really does perceive different qualities in moving objects which depend on motion but are not accounted for by the merely symmetrical relation of change of distance between them. But perceptible motion, like all that is perceived, is an appearance; it is not the motion that is measurable and to which it seems likely that there directly corresponds a real change in the spatial counterpart. The real motions that cause the perceptions of these appearances, being called by the same name, and being so intimately related to the former, are supposed by a kind of subreption to be absolute in the sense that each body has its own motion. When this view is worked out it leads to the theory of absolute space and motion that I suggested above. And that theory will explain the facts, and is not meaningless. On the other hand, a theory of purely relative real motion will equally explain the apparent absolute motions that people perceive. Hence the fact that common-sense quite rightly believes that the motions that it perceives are absolute is no reason why we should think that the motions with which we deal in science are so. The question remains a perfectly open one. And we have already come to the same conclusion from the experimental attempts at a proof of absolute motion. At the same time it ought to be added that the absolute theory lays claim to a much greater knowledge about the real than does the relative one, and therefore I think that, on philosophic grounds, the relative why? theory is to be preferred. For mathematical purposes the absolute view is undoubtedly better, and we have seen that problems treated in terms of absolute motion can be applied to the existing world when for absolute motion is substituted motion relative to axes defined by the fixed stars.

We have thus decided in the same sense as Mach, though by somewhat different arguments, that 'a straight line' for the purposes of the Laws of Motion means a straight line relative to axes defined by the fixed stars, and that 'uniform velocity' means velocity that is uniform relative to such axes[1]. But now the question arises: What do we mean by uniformity? We need a discussion about time and its measurement to answer this question. An uniform motion is presumably one in which equal spaces are covered in equal times. But what are equal times, and how do you know when they are equal?

We generally measure equal times by the rotation of the earth. But how do we know that the successive rotations of the earth take place in equal times? As a matter of fact we do not even believe it to be strictly true, for we talk of the slow change of the length of the second so measured owing to the tidal drag, and attempts have been made to calculate this change. It must be noted that this is not a question of whether time be absolute or relative; the question is one of measurement which applies equally to either theory, and rests on the fact that quantities of duration cannot be moved about in time as metre-scales can be in space and superposed. We thus seem to be in the same position as we should be in the measurement of space if we had to measure a length in England by considering how long it looks as compared with what we can remember as to the length of the standard metre in Paris from the time when we last saw it. Whether temporal relations be directly between events, or directly between absolute moments and only indirectly between events, the fact remains that it is only the relations between events that can be observed and measured, and so this difficulty arises on either view.

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[1] Subject of course to the possibility of defining axes by means of imaginary bodies such that motions of real bodies relative to them shall still more accurately obey the First Law.

Neumann[1] discusses this question. His method is (i) to state the First Law of Motion in terms of the *same* time instead of *equal* times, and then (ii) to construct from it a definition of equal times. On this view the actual statement of the first law becomes: 'Any two bodies A and B under the action of no forces describe straight lines relative to the body a, and successive equal lengths of A's path correspond to successive equal lengths of B's path.' You take any arbitrary distance on A's path and note where B is when A is at the beginning of this distance. You then note where B is on its path when A is at the end of this distance. Call these two distances l_a and l_b. You now let A run on to any other position, let us say l_a' from the point from which l_a was measured. When it reaches this point you note that B is at a position l_b' from the point from which l_b was measured. And you find that, if no forces act upon either, $\frac{l_b'}{l_b} = \frac{l_a'}{l_a}$. This then is Neumann's account of the Law of Inertia.

From it he now proceeds to give a definition of equal times. Equal times are those in which a body under the action of no forces describes equal lengths. Thus, the point of Neumann's definition is to replace equal times for one body by the same time for several bodies in the statement of the Law of Inertia. That law then states the relation between successive distances passed over in the same time by different bodies all unacted upon by forces. He then *defines* equality of time by means of the law of inertia so stated. This of course does not mean that he defines what is *meant* by the equality of two durations, but that he tells us what is to be taken as the measure of it in mechanics.

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[1] *Op. cit.* p. 18 *et seq.*

Streintz[1] criticises Neumann's position as follows. He says that Neumann's form of the law of inertia would still be obeyed if, under the action of no forces, any two bodies obeyed laws of motion of the form $x=\kappa f(t)$ and $x'=\kappa' f(t)$ where f is any function. This of course is obviously true. Streintz goes on to state that this is incompatible with Neumann's statement that equal times are those in which a body under no forces covers equal spaces. For, he says, all that we can tell from the fact that a body obeying Neumann's law of inertia has covered two spaces x_3-x_1 and x_4-x_3 which are equal is that

$$f(t_3)-f(t_1)=f(t_4)-f(t_3).$$

I think this criticism rests on a complete misunderstanding of Neumann's position. He is not trying to *prove* from his law of inertia that a body that obeys it will describe equal spaces in equal times. √He is *defining* the times in which such a body passes over equal spaces as equal. It is clearly impossible to refute a definition; the best you can do is to show that it covers some cases where the times are admitted not to be equal, or leaves out others in which we believe that the times are equal. But, with Neumann's definition, Streintz's hypothesis that $x=\kappa f(t)$ for a body moving under the action of no forces reduces to $x=\kappa t$. The fact is that Neumann's statement of the law of inertia is not simply convertible, and Streintz is quite justified in pointing this out; but, on the other, Neumann is also justified in taking a class of motions which the law says fulfil the conditions (viz. those performed under the action of no force) and defining the times which bodies animated with these motions take to cover equal spaces as equal: provided always that he can pick out this class of motions that are performed under the action of no force by some criterion other than their fulfilling the conditions laid down in the law.

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[1] *Op. cit.* p. 87 *et seq.*

The real criticism on Neumann's definition is that he does not tell us how to distinguish bodies under the action of no forces from others. His law of inertia, as we have now seen, cannot be a *criterion* of this until we have found at least one motion which is known, independently of that law, to be performed under the action of no forces. For his statement of the law merely tells us about the relation between the corresponding spaces passed over by two bodies both under the action of no forces. ?

Streintz[1] himself rests the measurement of time on an axiom, which is that, if any action takes place twice under exactly the same circumstances, it will always take exactly the same time to accomplish itself. This position needs a little further explanation before it can be criticised. The example that Streintz gives shows that he does not mean that the conditions are to remain constant throughout the whole of each example of the process. They may change during it in any way you like. But at corresponding points of each process the conditions must be the same. Under these conditions Streintz considers it axiomatic that the duration of the two examples of the process will be identical. The axiom is plausible, and we must examine it to see whether it may not contain an implicit circle. What precisely do we mean by 'corresponding points of each process'? Clearly not points at equal times after the beginning of each process; for this assumes the measurement of equal times. It must therefore be held to mean at the same observable stage of the processes. Thus, if we drop the same body from the same point above the earth's surface at different times equal durations will be supposed to have elapsed each time it reaches the earth, because it will be supposed to have been subject to the same conditions at corresponding points in its fall.

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[1] *Op. cit.* p. 80 *et seq.*

The axiom is not therefore logically vicious. On the other hand, are the circumstances ever the same in two different experiments, and, if so, how do we know it? It is perfectly clear that they never are precisely the same, and therefore the axiom, even if true, is useless as it stands as a means of telling when equal times have elapsed. It would need modification to the form: If any process can be repeated and, if at corresponding stages the relevant circumstances are the same, it will take place in equal times. But what are 'relevant circumstances'? Since *ex hypothesi* the two events as such do not differ in any observable characteristic the only way in which a circumstance can be relevant is to alter the time between any two corresponding stages in each of its successive occurrences. Hence to say that in corresponding stages no relevant circumstances are different, just means that in corresponding stages no circumstances are different that would make the time between a pair in one process differ from that between the corresponding pair in the repeated process. So now the definition of equal times based on the only form in which the axiom can be applied to their actual determination is sheerly circular.

Lange[1] gives a peculiar theory of the nature of the First Law of Motion both with regard to space and time. He asserts that his theory of time-measurement is identical with Neumann's in principle. This, as we have seen, would be a most serious objection to it if he happens to be correct in this statement. But his discussion is much more general than Neumann's and deserves a short consideration. It is best put in the second Appendix to the work with which we are dealing. Lange holds that the full statement of the First Law involves two conventions and two verifiable propositions. He says that you must not take the motions with which mechanics deal as being either (*a*) relative to absolute space and absolute time, or (*b*) relative to any axes defined by existing bodies or to actual events. He gives no new arguments for either position, and the only point that interests us at present is his discussion as to what these motions do involve. To answer this question he introduces an axiom called by him the 'Principle of Particular Determination.' I had better quote Lange's own words about this principle. 'Wir haben...ein Theorem, welches in Bezug auf eine beliebige Anzahl gleichartiger Elemente der wissenschaftlichen Betrachtung...die gleiche Behauptung...aufstellt. Der zum Verständniss dieser Behauptung vorauszusetzende Massstab der quantitativen Bestimmung...wird so normirt dass jenes Theorem für eines der Elemente gilt und zwar eben *per conventionem* gilt. Der Wahrheitsgehalt, d.h. das mehr als bloss Conventionelle des Theorems liegt dann nur darin, dass nach jener auf Grund *eines* Elementes erfolgten conventionellen Normirung des Massstabes das Theorem sofort für alle übrigen Elemente gilt.'

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[1] *Die geschichtliche Entwicklung des Bewegungsbegriffes.*

Now in the First Law we are told that all material particles left to themselves describe straight lines with uniform velocity. Lange takes any one particle left to itself and *defines* its motion as being one that covers equal spaces in equal times relatively to a certain set of axes. This is conventional. The times are defined as equal in which this particular particle covers equal spaces. The experimental part of the law is supposed to be that every other particle when left to itself describes relatively to the same axes equal spaces in times defined as equal by the motion of the first particle. It follows that the experimental law can be put in the form: 'If out of all the particles moving under the action of no force any one be chosen, then every one of the rest will describe equal spaces whilst the first describes equal spaces, and the times in which the first does this are defined as being equal.' This is practically the same as Neumann's account of the law of inertia. The definition of equal times as those in which the assigned particle describes equal spaces only differs from Neumann's definition by referring to an assigned particle instead of to any particle left to itself. I fail to see that this is any improvement. In fact the whole method of treatment suffers from the same defect as Neumann's. It has to assume that you can know when a particle is under the action of no forces, and it offers no test by which this can be determined without assuming that you have been able to determine it independently in at least one case. Lange's attempt to define a straight line for the purposes of the first law of motion seems to me to labour under the same defect and some others besides. If I understand him aright his position is as follows. If three material points be shot out together from a single point of space with their directions not in one plane and left to themselves they will in general describe curves. You can prove as a mere matter of geometry that there is always at least one set of axes relative to which all these curves are straight lines. The physical part of the law is the further assertion that any fourth material point left to itself will describe a straight line relative to axes so defined. Clearly we have the old trouble about how you are to judge when material points are left to themselves apart from the laws of motion. Moreover the statement that the three points are left to themselves is ambiguous. Does it mean left to their mutual actions but free from those of any other bodies, or that each is free from the action of all other bodies? Lange might reply that it is of no importance whether we can tell in particular cases that bodies are left to themselves, because his object is not to point out an actual inertial system (this can only be done by experience) but to define what is meant by such a system. But this does not meet the objection that the notion of being 'left to itself' enters into the definition of the system and therefore must presumably be relevant, and yet it must mean 'left to itself' not in some vague general way but in a strictly mechanical sense, and we can only give a meaning to this *after* we have stated and accepted the laws of motion.

The question then arises: Have we not some measure of the equality of the duration of two events which do not completely overlap, independent of the laws of motion? It is perfectly clear that people did measure times and did judge them to be equal or unequal before the First Law of Motion was thought of; and there must therefore be some supposed criterion independent of any assumption as to the presence or absence of forces. Streintz's axiom, we saw, would not work as stated, but it seems to be much more promising than any that we have yet noticed. I think, however, that the best account of the criterion of the equality of non-coincident durations is to be found in an article in *Mind*[1] by Rhodes. It will not do as it stands, but by introducing certain modifications I think it can be made fairly satisfactory.

Recurrent events are either isochronous or anisochronous. We want a test for isochronism. Rhodes says that there is no intrinsic difference between a series of isochronous events and one of anisochronous events. He goes so far as to say that there is nothing in the motion of a pendulum taken alone to make us believe that it is any more likely to be isochronous than are the motions of a weather-cock. This, I think, is exaggerated. It does not seem to me that we lack all power of making judgments correctly about the relative magnitudes of durations that do not overlap completely. The trouble with us is that these judgments are very likely to be affected by subjective factors, and that to a much greater degree than immediate judgments about spatial equality or inequality. Further, whilst we can make our immediate judgments about space almost indefinitely more accurate by means of measuring-rods—though the assumption of their constancy of length raises rather difficult problems—we have not as yet found anything that corresponds to a measuring-rod for time. It is for this purpose that we want isochronous recurrent processes. Thus, whilst I differ from Rhodes in believing that anybody in his senses would recognise that a pendulum is very much more nearly isochronous than a weather-cock by merely looking at the movements of each, I agree that we want something more than the intrinsic qualities of the pendulum's motion to assure us that it really is isochronous. It might be much more nearly so than the movements of the weather-cock and yet be far from perfect isochronism.

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[1] *Mind*, O. S., Vol. x, 1885, 'The Measurement of Time.'

Rhodes's method of determining isochronism is as follows. Suppose there is a process P whose isochronism we want to test. Let P' be any other process that goes on contemporarily with P. Then, if P' be recurrent, it is either isochronous or anisochronous. Thus the *à priori* possibilities are (i) P and P' both anisochronous; (ii) P and P' both isochronous; (iii) P isochronous and P' anisochronous; and (iv) P anisochronous and P' isochronous. Now in case (ii) it is certain that to every complete recurrent event in the process P there will correspond the same number of events in the process P'. You cannot of course ensure that it shall be a whole number, but it will always be the same whole number + the same fraction of an event. By a fraction of an event we mean an example of one of the recurrent events that reaches a certain determinate stage other than the end one of it. This is a necessary characteristic of two isochronous recurrent events, whether there be any causal connexion between them or not. Does it completely distinguish the case (ii) from all the other possible cases! It is clear that it cuts out the cases (iii) and (iv) entirely. We are left then with (i). Rhodes of course admits that two anisochronous series of events might have this quality which two isochronous series must have. Hence the condition for isochronism that he has given, though necessary, is not sufficient. He therefore adds that the two processes P and P' must be 'independent of each other.' Unfortunately he does not tell us how this is to be discovered, and so his theory becomes useless.

I think, however, that we can complete the criterion in terms of the theory of probability, on the assumption that we can make rough immediate judgments about the equality or inequality of durations that do not entirely overlap. For the present purpose the statement that P and P' are both isochronous but are not independent means that each is accelerated or retarded according to the same law. Now it is fair to assume that the more different the two processes are from each other the less likely is this to be the case. It is not *à priori* wildly improbably that all pendulums are actually retarded according to the same law even when swinging *in vacuo*; but it is very unlikely that the rotation of the earth, the swings of a pendulum and the vibrations of an electron are all retarded according to the same law. Let us call the proposition 'P is isochronous' p_ι. Let us denote the contradictory of a proposition q by $\bar{q}$. Let us call the proposition that to an event in P there always corresponds the same number of events in P', r. Finally I shall adopt the notation $p/q =$ 'the probability of p given that q is true.' $p/f =$ 'the probability of p given the knowledge that everyone is supposed to have before the experiment is performed.

We want to compare p_ι/r with $\bar{p}_\iota/r$. We know that

$$r/p_\iota \bar{p}_\iota' = 0, \quad r/\bar{p}_\iota p_\iota' = 0, \quad \text{and} \quad r/p_\iota p_\iota' = 1.$$

Now $$p_\iota \equiv p_\iota p_\iota' \,.\, \text{v} \,.\, p_\iota \bar{p}_\iota';$$

$$\therefore \quad p_\iota/r = p_\iota p_\iota'/r + p_\iota \bar{p}_\iota'/r$$

$$= \frac{p_\iota p_\iota'/f \times r/p_\iota p_\iota'}{r/f} + \frac{p_\iota \bar{p}_\iota'/f \times r/p_\iota \bar{p}_\iota'}{r/f}$$

$$= \frac{p_\iota p_\iota'/f}{r/f}.$$

Again

$$\bar{p}_\iota = \bar{p}_\iota p_\iota' \,.\, \text{v} \,.\, \bar{p}_\iota \bar{p}_\iota',$$

$$\therefore \quad \bar{p}_\iota/r = \bar{p}_\iota p'_\iota/r + \bar{p}_\iota \bar{p}'_\iota/r$$

$$= \frac{\bar{p}_\iota p'_\iota/f \times r/\bar{p}_\iota p'_\iota}{r/f} + \frac{\bar{p}_\iota \bar{p}'_\iota/f \times r/\bar{p}_\iota \bar{p}'_\iota}{r/f}$$

$$= \frac{\bar{p}_\iota \bar{p}'_\iota/f \times r/\bar{p}_\iota \bar{p}'_\iota}{r/f}.$$

Hence we have that

$$\frac{p_\iota/r}{\bar{p}_\iota/r} = \frac{p_\iota p'_\iota/f}{\bar{p}_\iota \bar{p}'_\iota/f \times r/\bar{p}_\iota \bar{p}'_\iota}.$$

Let us now consider this fraction critically. The first point to notice is that all the terms in it, being values of probabilities, must be proper fractions. Hence there is a general expectation that the denominator which is the product of two fractions will be pretty small. Further, from what we have already said about the possibility of judging roughly of isochronism by immediate inspection, and in view of the fact that the experiment is being used on two processes that appear to be isochronous to see whether they really are so, we see that $p_\iota p'_\iota/f$ is very much greater than $\bar{p}_\iota \bar{p}'_\iota/f$. It follows that p_ι/r must be very much greater than $\bar{p}_\iota/r$ unless $r/\bar{p}_\iota \bar{p}'_\iota$ is practically equal to one. But it is perfectly obvious that $r/\bar{p}_\iota \bar{p}'_\iota$ is a very small fraction, since it is only under very special circumstances that two non-isochronous series of events should be so connected that to every complete event in one there shall correspond the same number of events in the other. It is clear from observing series of events which immediate inspection shows to be anisochronous that in general this connexion does not hold. And from what we have said above it is clear that the less alike the two processes P and P' are to each other the smaller is $r/\bar{p}_\iota \bar{p}'_\iota$, for the less likely it is that both are accelerated or retarded according to the same law. On the other hand the fact that they resemble each other little in other respects is no reason for thinking them unlikely to be really isochronous if they seem to be so, i.e. for $p_\iota p_\iota'/f$ to be small. Hence we conclude that if two series of recurrent events appear to be isochronous by themselves, and if to each event in one there always corresponds a fixed number of events in the other, it is enormously more probable that each is isochronous than that neither is so, whilst it is impossible that one should be isochronous and the other not. I think therefore that Rhodes's method of recognising isochronism is really satisfactory and non-circular with the modifications that we have made in it. There are two further considerations not mentioned by Rhodes which may be added touching the effect of the duration of the experiments. (i) If there be the smallest difference between the laws of two anisochronous processes it is certain that the longer they are compared the more it will show itself. So that in the case of two processes which give the test for isochronism however long we try them together we are forced to decide either for real isochronism or anisochronism + exact identity of law. And the latter is much less likely than the former, as we have said, if the two processes be of very different kinds. (ii) If we consider a single recurrent process and find that (*a*) it recurs pretty quickly, and (*b*) that it continues as long as we care to observe, or at any rate for a very long time, and (*c*) that successive occurrences seem to us to take the same time, we can get a fair test of equality of time. For (*a*) and (*b*) tell us together either that the process is not retarded at all or that the retardation is very small at each recurrence. And (*c*) tells us that it must be pretty regular. But if two successive occurrences are judged to take the same time and it can be proved that the retardation in each must be very small we must be committing a very small error in counting them as equal. Thus even a single process which is actually not perfectly isochronous may when properly criticised give us a very accurate measure of equality of two times, provided we admit that we are capable of comparing pretty accurately the times taken by two successive events of the same kind when the time occupied by each is short. And there seems no reason to doubt this.

We can now define equal times as those in which a recurrent event takes place in an isochronous recurrent series; isochronism being measured according to the method indicated above. Since this method makes no mention of forces we can state the Laws of Motion without any further trouble as regards space and time. The first law will run as follows: A body under the action of no forces describes a straight line relative to axes determined by the fixed stars so that, whenever it covers equal spaces, the same number of events takes place in some recurrent series which appears to be isochronous and is found to be so related to some other series that appears to be isochronous that to every event in the former there corresponds the same number of events in the latter. It must be added that, whilst it is perfectly possible that a series which seems isochronous and fulfils the conditions with respect to another apparently isochronous series might not obey them when tested with respect to another apparently isochronous series, yet, as a matter of fact, this does not generally happen. Most perceptibly isochronous series obey the test indifferently with each other within the limits of observation; and this is what we should expect from real isochronism, whilst we should not expect this agreement if we were mistaking for isochronism connected anisochronous series. But, when once the science of mechanics has been based on the laws of motion, and these upon this method of measuring equality of time, it is perfectly possible and logical to show that the world will be more easily and fully explained if we assume certain small and imperceptible departures from isochronism in processes on whose assumed absolute isochronism the laws of motion were originally built[1].

The remaining problem of the laws of motion is that of Force. What is meant by force, and how do we know whether it is present or absent? It would seem that, in order to arrive at the law that when no forces act on a body it moves in a straight line with uniform velocity relative to a certain set of axes, we must have been able to recognise, in some cases at least, the presence or absence of force by other characteristics than those of motion. To discuss this point it will be best to turn to the Second Law of Motion. This law offers us a measure of force and not a definition of it. It says that the force acting on a body in a given direction at a given moment is measured by the rate of change of its momentum in that direction at that moment with respect to time. Momentum is measured by the product of mass and velocity, and, if mass be constant, this will mean that the measure of the force acting on a body at a given moment is the mass × the acceleration at that moment in the given direction. This is supposed to be relative to the axes and the measure of time that we have already discussed. It follows at once from this that, if the velocity be constant in a given direction, the force will be zero in that direction. Hence the converse of the First Law follows from the Second. The law itself does not follow however. The fact that when a body is at rest or moves with uniform velocity in a given direction the measure of the force in that direction is zero does not prove that, when no force acts on a body, it will move in a straight line with uniform or zero velocity. We must therefore discuss the Second Law in order to come to any conclusion about the nature of force. On the subject of the position of force in mechanics a long discussion has raged; some holding that it is necessary to retain it, others that it ought to be rejected as a mere relic of animism. We cannot perhaps expect to say much that is new on such a hackneyed subject, but we cannot pass it by without discussion.

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[1] This may be compared with our desertion, first of the earth, and then perhaps of the fixed stars, as axes of reference relative to which the First Law holds, and the substitution of axes defined by imaginary bodies so determined that motions relative to them shall accurately obey the First Law.

The whole question of force is closely bound up with the view that is taken of causation. Kirchhoff and Mach who banished forces from mechanics, supposed, it would seem, that the notion of cause is obsolete, and that the case of force in mechanics is a peculiarly disgraceful example of the survival of obsolete prejudices. We should prefer to say that force is bound up with the activity view of causation, although it will be obvious enough that we do not hold that, with the departure of activity, science must or can drop causation. I think that, as far as Mach is concerned, any lengthy discussion on this point would be merely verbal. It is quite clear that, rightly or wrongly, he retains what we have called causal laws, and what we have held to be practically the whole of causation. In this sense then Mach does not drop causation from Mechanics. On the other hand he does drop the activity view of causation, and he seems to hold that the name 'cause' is no longer applicable after that view has been dropped. Any argument on such a question as this would obviously be merely verbal; and it is not worth while to embark on it. We have merely to note that, in our sense of causation, there undoubtedly is causation in mechanics, and that we mean to go on using the word.

The classical definition of force is that it is the immediate cause of motion. But this is far too vague. For motions with a uniform velocity can and do continue without the action of force. We should have to say that it is the cause of acceleration, instead of merely saying that the acceleration at a given moment is the measure of the force at that moment. But it is clear that people did not mean that everything that caused an acceleration was a force in the sense in which mechanics uses the term. When I will to move my arm it might be said that my volition is the cause of the motion. But it would not be said that a volition was a mechanical force. Yet again when one billiard ball hits another it would be said that the first causes the acceleration of the second. But no one would call a billiard ball a force. We saw that the activity view of causation makes the cause a substance in a certain state, and the effect a state of a substance. Hence I think we can fairly state the common view of force in mechanics as follows: A body in a state of exercising force is the cause of a state of acceleration in another body.

Now we have already rejected the activity view of causation which makes substances in a certain state the causes of states in other substances. We cannot therefore make any exception in the present case. Force seems here, as activity is everywhere, to be a mere *qualitas occulta*, the mere turning of a causal law connecting observable or inferrable states of two substances at different times into a quality of one of them. But the case for force can be argued a little further by its upholders. Some people think that some of their perceptions are either perceptions of force, or, at worst, of something that proves the existence of peculiar qualities to which the name forces may appropriately be given. If this were true forces would not be occult qualities and we could state laws, in our own sense of causation, about them. We must therefore enquire for a moment into these alleged perceptions of force.

We may agree with Höfler[1] that the term force is ambiguous in ordinary life. It covers tensions and pressures and also supposed permanent real qualities of bodies, such as the force of gravitation. Höfler adds a third meaning, viz. mass-accelerations. This is not even I think what is *meant* by force except by people who merely wish to keep the name force after they have given up the common belief in it as something special; it is certainly not the idea that the word force calls up in the mind of any unsophisticated person. The people who wish to identify force with mass-accelerations seem to me to have forgotten that there is such a thing as the science of Statics where forces are believed to operate as much as in Dynamics but where there are no accelerations. Hence, I think, we shall do best to say that, if there are forces, then they are the causes of mass-accelerations in dynamics and of the straightening of strings and the extension of spring-balances in statics. The question is whether people are right in thinking that the qualities called forces can be perceived, and whether they have any special perceptions which tell them independently that there must be these special qualities even though they cannot perceive them.

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[1] *Studien zur gegenwärtigen Philosophie der Mechanik*, p. 31.

It is obvious that perceptions of straightened strings, or extended spring-balances, or accelerated bodies are not themselves perceptions of forces. And it is by no means clear that the causal laws which explain such perceptible effects ever need among their data any special qualities like forces, or indeed anything but positions, motions, and certain characteristic coefficients. If there be any perceptions whose objects can reasonably be held to be forces or to necessitate a belief in special qualities which we can call forces as their cause they must be our sensations of tension and pressure. The names of these obviously beg the question of whether their objects really are forces. But it is tolerably obvious that, when we consider, we do not hold that they are any more preceptions of forces than are perceptions of stretched strings or accelerated masses. When we feel our skin pulled or pressed we localise the felt pull or pressure in our skin as we should a felt temperature. But, if we suppose that forces are qualities which are localised somewhere in the body that is causing the pull or pressure, since that is not in general our own body, it is clear that the immediate object of our perception is not what is believed to be the quality of force.

Indeed when we say that we feel a pull we mean that we have a perception whose object is localised at a certain part of our body and which is supposed to be caused by a force in another body which is pulling it. We must say then that common-sense holds, not that it directly perceives forces in its experiences of pulls and pressures, but that it perceives a quality localised in the body the existence of which is supposed to be a mark of the existence of force in the body that is said to be pulling or pressing.

But, after all, is there any better reason to assume a new kind of quality as the cause of these perceptions than for the cause of the straightening and stretching of strings? On the contrary it seems quite clear that these perceptions are the result of the shortening and lengthening of parts of our skins. They thus depend on the general effects in bodies with regard to which we saw no good reason to suppose that it was necessary to assume forces, as special qualities of bodies, as their causes. Hence these feelings cannot furnish the smallest additional evidence for the necessity of assuming real forces.

The only other experiences that we have which might seem to offer any evidence for the reality of forces are sensations of effort that we have in pushing, pulling, and raising bodies. We must not confuse these with the sensations that we have already discussed. The two always occur together, and this might be expected, if there really were forces and these sensations were closely connected with them, from the Third Law of Motion. It is, however, perfectly easy to distinguish the two. Can we say that the perceptions of effort are the perception of the quality of force in our bodies? I do not think that Poincaré's rhetorical question: 'Do you suppose that the sun feels muscular effort in keeping the planets in their elliptical paths?[1]' has any particular weight, although he thinks that it settles the matter. Naturally if the sun has no mind it does not perceive anything at all. But that is quite irrelevant. The true question is: Does the sun in keeping the earth from moving off at a tangent have that sort of quality which we who have minds perceive when we forcibly drag a heavy body? Is it a real quality, and, if so, may it not be a quality that only belongs to organic bodies? Even if we conclude that it is a real quality in our own bodies it is a sheer inference to suppose that it exists in other bodies as different from our own as the sun or a billiard-ball. And, as in the case of temperature, such an inference is very precarious.

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[1] *La Science et l'Hypothèse*, p. 130.

I conclude then that we have no reason to suppose that inorganic bodies, with which, after all, we are mainly concerned in mechanics, have any qualities in them comparable to what we perceive in our own bodies when we say that we exercise force or have force exerted upon us. Such qualities cannot be directly perceived in other bodies, the inference from our bodies to non-organic ones is precarious in the extreme, and there seems every reason to believe that the causes of these peculiar perceptions in us are not special qualities called forces in foreign bodies, but the same qualities and states as cause the tensions of strings and the changes of velocity in bodies. It only remains then to ask whether the states and qualities that cause these various effects are something special to which the name of forces can be given, or are just the positions, velocities, etc., of bodies. What we have now seen is that certain peculiar experiences of ours give no reason for adopting the former alternative.

Mr Russell has an argument which, if true, would demolish forces altogether as real qualities. We have seen that, if forces be real, they must be the causes of accelerations among other effects; though it does not of course follow that the causes of accelerations must be forces; for that is the question that we are now discussing. Now Mr Russell's argument is that an acceleration is not an event and therefore cannot be an effect. Hence it cannot have a cause. Hence, if there are no causes of accelerations and forces are the causes of accelerations or nothing, there can be no real forces. Let us then consider Mr Russell's arguments to prove that accelerations are not real events. There are two of them, one narrower and one more sweeping. The first argument[1] is that, whatever may be the case with a *resultant* acceleration, it is certain that a *component* acceleration is not an event. The components of a vector are merely the products of analysis. The vector from London to Cambridge can indeed be resolved into a vector from Euston to Bletchley and another from Bletchley to Cambridge; but a sane person in coming to Cambridge from London does not pass through Bletchley, and so the journeys represented by these two vectors are not in any sense implied in the journey represented by the vector from London to Cambridge. This is the only one that is actually made. The others are not *parts* of it which must exist if it does, but operations whose final result, *if* they were made,—which they are not—would be the same. Hence component forces at any rate cannot be real. The other argument[2] is that no acceleration, component or resultant, can be a real event. This argument is based on Weierstrass's account of the nature of a differential coefficient. The momentary acceleration is the differential coefficient of space by time for that particular value of the time. As such it is a limit and a mere number, and so cannot be an effect of anything. Hence that which, if it were anything, would be the cause of an acceleration is a mere nothing and cannot be real.

These arguments are interesting, and, if a belief that accelerations are real events be truly incompatible with the theory of the nature of a differential coefficient, it must be rejected, since that theory is undoubtedly true. But I do not think that there is this incompatibility. No doubt an acceleration as a limit is a mere number, but then no one ever supposed that, by saying that the acceleration of a body at a given moment was x feet per second per second, he meant that it was *identical with* x feet per second per second. I can see no reason whatever why velocity and acceleration should not be real states of bodies with magnitudes measured by the numbers which are reached as the limits of certain mathematical expressions. Whether an acceleration be a real event or not is of course another question; but I do not see that the fact that, if it were, its magnitude would be measured by the value of a limit could prove that it could not be real. It will be well to make it quite clear that this possibility interferes in no way with Weierstrass's solution of the Zenonian paradoxes about motion. The essence of that solution is the insistence on the fact that every value of a variable is a constant, so that at every moment the arrow in Zeno's paradox is at one definite point equally, whether in a finite time it moves or not. The difference between the two cases is simply that, in the one, it is at different points at different moments, whilst, in the other, it is at the same point at different moments. Now this would meet all the difficulties just as well if at every moment the moving arrow had a quality with an intensive magnitude measured by the differential coefficient of space with respect to time for the body at that moment. The moving arrow would still, at any moment, be at a single point of space, and, at different moments, at different points. It would merely have a quality at each point which the arrow that was at the same point at different moments would not have. Hence Weierstrass's unquestionably correct account of motion and his denial of infinitesimals does not touch the possibility of the existence of a physical quality of velocity or acceleration; and I cannot agree with Mr Russell that Weierstrass's theory *necessitates* a more integrated form of the laws of motion.

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[1] *Principles of Mathematics*, pp. 484, 485.

[2] *Ibid.* pp. 473, 474.

We pass then to the other argument that, at any rate, only resultant accelerations could be real. It is quite clear that Mr Russell's argument is valid as regards the ordinary analysis into components in three directions at right angles to each other. These three directions are chosen merely for convenience in handling the problem under discussion, and correspond to nothing that actually takes place. But is it certain that nothing corresponds when the components are apparently actually given? For instance, in the case of a system of gravitating particles, are there no accelerations along the lines joining the given particle to each of the others? It is quite certain that there are no *motions* along these lines in general, but only along that of the resultant force on the particle at each moment. It is also true that in a finite time τ the position reached by the particle in question is not what it would be if it were successively dragged along the lines joining it to the particles at the beginning of the motion for a time τ along each. If we grant, however, that accelerations may be states of particles, whose magnitude is measured by the differential coefficient of space by time at the moment for the particle, then it would be possible that at each moment the various particles did produce these states with their appropriate magnitudes, even though no states corresponding to an arbitrary selection of components existed. But, although I do not think it possible to disprove the existence of such states, I see very little evidence for their reality. And it is certain that the laws of mechanics can be stated without them. Hence it seems much more philosophical to state any causal laws that are involved in a form that avoids velocities and accelerations. At the same time you cannot work out any problems in mechanics in this integrated form. You must also remember that, although a statement of the relations between accelerations is not directly about what exists, yet the fact that such statements bring you back in the end to what does exist and can be observed makes them laws about the existent. It was no doubt important for Mr Russell to show that, if you insist that causal laws shall only connect actually existent states or events, then the laws of motion will involve relations between configurations at three moments. But, when you have passed so far away from the ordinary view of causation as Mr Russell has done and as we have also, it seems rather needless purism to deny the name of causal laws to uniformities that connect, if not existent states, yet terms which only are connected as they are in virtue of the nature of the existent.

But, even when we state laws in terms of accelerations, we do not need to make any mention of forces. Hence we can conclude that, if accelerations do not exist, forces, as causes of them, cannot exist; whilst, if they do exist, there is not the slightest reason to suppose that their causes are special qualities called forces. Wherever forces nominally occur in dynamics it is always in the form of special laws over and above the laws of motion, connecting the configuration or states of a system with the accelerations of its parts. Thus, when we talk of the electrical attraction between two charged particles, we seem to be talking of force as a cause of their accelerations. But, when we work out a problem, the only way in which the alleged force enters is in the fact that coefficients e, e' are to be attached to each particle and that the acceleration in one due to the presence of the other is $\frac{ee'}{mr^2}$, where r is their distance apart. Instead of the force determining the acceleration it is determined by two coefficients and the relative positions of the particles according to a certain law. The only use that force has in dynamics is that it is a convenient phrase when we do not want explicitly to take into consideration the other bodies in the system. We can then talk of a field of force; but we merely mean by this that bodies placed in a certain position all have a certain definite acceleration imparted to them that only depends on their own masses and other coefficients.

Our best plan will now be to try and state the laws of motion without force, although not necessarily in terms of configurations, for the reason that I have given above. For a reason into which we shall enter later the laws must be stated in terms of particles and not of definite bodies.

I should state the First Law in the following terms: 'Changes in the magnitude or the direction of the velocity of any particle, relative to axes determined by the fixed stars, and measured by processes shown to be isochronous by the method discussed above, cannot be determined by purely immanent causal laws.' The first law then tells us that we must look for the other data in any causal law that shall enable us to predict such changes relative to the fixed stars in events or qualities of something outside the system consisting of the particle and the stars. Is the evidence for this law empirical or *à priori*? The only possible way to show that it was *à priori* would be to connect it with our axiom about causality, and it is easy to show that it cannot be deduced from that. Our axiom, it will be remembered, said that, if any system had been quiescent for a finite time and then began to change, the change could only be explained by a causal law transeunt to the system. It is clear that this is both too wide and too narrow for the first law of motion. It is too wide in as far as it tells us that in *any* system of particles that had been relatively at rest for a finite time the cause of any change from rest must be looked for outside the system. For our law refers to a particular system, viz. that of the particle and the fixed stars. And to that system it does not apply because we know that the system of the fixed stars and any particle is not quiescent, since the fact of stellar motion in the line of sight renders it almost certain that the stars are in relative motion to each other. But again it is too narrow. At best it could only tell us that a particle at rest relatively to the fixed stars would remain at rest; it could not tell us that a particle in motion with reference to them might not move in circles, or stop, or perpetually alter its velocity without our having to go outside the system for explanation.

If there be any evidence then for the law it must be empirical. Everyone knows the type of direct empirical evidence that is offered for the law. We are told that if we throw a stone along a smooth flat surface it will travel a long way in a straight line, and that the smoother the surface is made the further it will travel. This always appears to me to be a bad example of the law, though I think it is of importance in view of the historical prejudices which the law had to meet. With regard to the first point, it is a bad example because we need to commit a circle in order to be sure that it is an example at all. The reason is as follows. What do you mean by smooth? If you mean a geometrical quality then it is clear that the necessity of mentioning it in the example shows that you have not reached an immanent causal law. The future position of the body is foretold, not from its present position and velocity alone, but from these + the geometrical qualities of the surface on which it moves. Hence, as it stands, we have no example here of the supposed law. But if, on the other hand, you mean by smooth that the surface is one that has no effect on the motion of the body, then the experiment and its completion in thought merely come to this, that continued movement in a straight line is a proof that the surface is not having any causal influence. But it is only a proof by assuming the law, and so the experiment can give no support to the law.

The fact is that there is no non-circular direct experimental evidence of the first law. What an experiment of the kind just discussed did do was to upset the scholastic theory of motion based on the maxim; *Cessante causâ cessat effectus.* The application of this maxim to motion was that a motion only continued as long as that which originally caused it continued to act, or at any rate, that, when it ceased, the motion died away of itself. Now these views the experiment does tend to undermine, and so it makes way for the true view. When it is seen that the stone goes on for some distance and the distance depends on the nature of the surface it suggests that motion does not have to be kept up by the same causes as produce it, since a flat surface is not a cause of motion in the first instance. And, when it was further seen that the more you polished the surface the further the stone went, it followed that it was much more reasonable to ascribe the gradual cessation of motion to the nature of the surface than to some immanent causal law in the nature of all moving bodies. Thus, the value of the experiments that are alleged to prove the first law is not that they do prove it but that they render improbable certain plausible alternative views that are incompatible with it.

The true proof of the law is to be found in the explanation that it offers of projectiles' paths and of planetary motion. It was found that, under certain circumstances, by holding that motions were compounded out of unchanging velocities in certain directions and changeable ones that obeyed definite laws in which the presence and qualities of other bodies were data, these motions could be completely accounted for. And wherever the velocities did remain constant in magnitude and direction no mention of the states of other bodies was needed.

With regard to the second law I agree with Mr Russell's statement[1]. This is that, in an isolated material system of n particles, there are certain constant coefficients called masses (m_1, m_2...m_n) to be associated with the particles respectively. Then, for any particle in the system, the acceleration multiplied by the mass is a function of the masses, relative positions, and velocities of the particles at the moment. It will be the same function for the system at all times. And in general it will involve other coefficients beside masses.

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[1] *Principles of Mathematics*, p. 483. [It might seem that the statement that it was the *momentary* configuration that is relevant is contradicted by the form of Hooke's Law for stretched strings within the elastic limit, viz. $m\ddot{x} = -\frac{q(x-x_0)}{x_0}$ where x is the present and x_0 the unstretched length. But, in a system so isolated as Mr Russell takes, we could hardly talk about the unstretched length of a body. We could merely say that x_0, like q, is a constant that has to be introduced in connecting the momentary configuration with the momentary acceleration.]

There are two points to notice about this law. In the first place the other coefficients that enter into the expression are the marks of what are called 'forces of nature' such as gravitation, and electrical charges, and the elasticity constants of bodies. We have already seen that they are not anything that need lead us to believe in forces in the old sense. What is called a 'mechanical explanation' consists in replacing them by masses, positions, velocities, and geometrical conditions. We shall have to discuss the meaning of such attempts toward the end of this chapter.

The other point to notice is that the law is only asserted for an isolated system. When we ask what this means we find that it means a system whose states at all moments can be determined by a purely immanent causal law from a sufficient number of observations made within it. How wide a system must be in order to be isolated is a question that cannot be answered *à priori*, but we learn by experience what kind of terms are relevant and what are not. All systems, if our discussion on space and motion be correct, must be assumed to include the fixed stars, or, at any rate, axes defined by bodies that are known to be at rest or in uniform translational motions in straight lines with respect to them.

The nature of the evidence for the Second Law is once more entirely indirect. It is in fact precisely the same evidence as proves the first law, viz. that all mechanical processes can be analysed in this particular way. As Poincaré points out, if they appear to fail, you can always avoid the difficulty by assuming enough hidden particles in proper positions and motions to which these two laws do apply.

To discuss the notion of mass we need the third law. Now it is with the third law that the introduction of particles instead of finite bodies becomes essential, so that this will be the best place for the discussion of this question foreshadowed on p. 333. The third law states that, in an isolated system of particles, you can take the particles by pairs, and, considering their accelerations along the lines joining them, these will always be opposite in direction, and the product of them by the respective masses of the pair will be equal. So far we have only spoken of masses as coefficients introduced in the second law and apparently comparable to such other coefficients as e, the electric charges, or γ, the gravitational constant. But here we learn something fresh about the coefficients. Granted that for any two particles in the system f_{rs} and f_{sr} are opposite in sign, which is a law of nature, it is of course a mere convention that coefficients m_r and m_s can be found such that $m_r f_{rs} + m_s f_{sr} = 0$. But where we pass out of the world of convention and back to laws of nature is in the fact that, if we take any third particle, and, with the same notation as before, have $m_r f_{rt} + m_t f_{tr} = 0$ where m_t is merely chosen to make this equation hold, we shall find that, with the value of m_t thus reached, the equation $m_s f_{st} + m_t f_{ts} = 0$ holds. And you will find that, when the relative masses of the particles in a given isolated system have thus been determined, they are, in general[1], fixed and independent of any other variations in the states of the particles.

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[1] This is not strictly true of moving charged particles, when the velocity approaches that of light.

Now it is quite clear that such a proposition as the Third Law has no definite meaning except for particles, because there is no line that can be called *the* line joining two finite bodies. At the same time all that can ever be observed is finite bodies and their states. How then do we reach the notion of particles and the laws stated in terms of them? And why do we state the other laws also in terms of particles and not of finite bodies?

The analyses of motion which, I have argued, led to the first and second laws were analyses of the motions of bodies of finite size, rigid, and moving translationally. Such were the experiments with projectiles, and falling bodies, and the simple pendulum; such too were the motions of the planets about the sun to which Newton first applied this analysis.

It is, of course, true that the earth and the other planets also rotate on their own axes; but Newton found that it was not necessary to take this fact into account when he laid down the laws that govern the motions of the planets about the sun, and of the moon about the earth. Hence something very like the first two laws of motion was reached without having recourse to particles. They were not indeed directly observed to hold of finite rigid bodies in their translational motions, but they could be analysed out of those motions. Similarly, owing to the large distances of the planets from each other and from the sun, it was possible for many purposes to treat them as points and talk of *the* line joining them. This enabled the third law to be applied to them in a rough way, but it was not their motions that were the evidence on which the third law was based. That evidence was of two kinds. It came partly from dynamics and partly from statics. Also, whilst it is much the most paradoxical of the three laws to the untutored mind, it is curious that it is the only one of the three for which there is a kind of direct sensible evidence. The dynamical evidence for the law with regard to finite bodies came from the experiments of Huyghens and Wren on the percussion of spheres.

They were not, of course, entirely clear as to the distinction between mass and weight, but then weight is an accurate enough measure of mass at a given place on the earth. The result of their experiments was to show that, when the spheres ran into each other along the line joining their centres, the sum of their momenta before and after impact was constant, whatever might be the elasticity of the bodies. Owing to their use of spheres they could get a clear account of what was meant by the line joining the two bodies at the moment of impact. When we combine this result with the second law of motion we see that the accelerations of the bodies which ended in the permanent change of their velocities, and presumably took place while they were in contact, must have been in opposite directions along the common normal, and must have obeyed the third law. Similarly, dynamical experiments can be performed with floating magnets which support the law for finite bodies that can be treated as points.

I will not interrupt the argument for the moment with the consideration of the statical proofs, and the direct experiences of the truth of the law for finite bodies, but will pass at once to the introduction of particles from what was already known. What was already known then was that the three laws held for the translational motions of bodies which were rigid and approximately spherical like the planets and the balls in the impact experiments. Here you could give a definite meaning to *the* velocity or acceleration of the body in a given direction, and to *the* line joining two bodies under certain circumstances. But, with such limitations, it is clear that there is an immense number of motions with which Newton's laws could not deal. They could do nothing with pure rotations, since they are all about translational motion. They could not be directly applied to bodies that are not fairly rigid, for how can you talk of *the* velocity or acceleration of a body whose different parts have different ones. And, except for bodies so far apart with respect to their size, as to be treated as points, or bodies that only act when in contact where you can define directions by the common normal at the point of contact, how can you give a meaning to the Third Law?

The passage to particles from finite bodies is best seen in the treatment of rotation which first showed that Newton's laws could deal with motions of that kind as well as with the translational motions in reference to which they were formulated. The first problem involving the rotation of rigid bodies that was solved was that of the time of swing of a compound pendulum. This was first fully discussed by Huyghens. Huyghens' solution, however, was based on principles which, though compatible with Newton's, and indeed, mutually deducible from them, were formally different. It involved, in fact, the conceptions of work and kinetic energy instead of those of force and momentum. The statement of the Newtonian method of dealing with such cases is to be found in D'Alembert's principle.

But the essence of both solutions is to imagine the body divided up into an indefinite number of masses located at geometrical points. You thus get rid of rotation on the part of these elements, because a point cannot rotate. On the other hand, you can replace the rotation of a finite body about an axis by the translatory motion of its elements in closed curves. Now the laws of motion, as we saw, were obtained from just such motions of finite bodies. It is then obviously worth while to enquire whether, if you assume that their translatory motions obey the laws of motion for finite bodies, you will be able to account for the motions of the whole system. At any rate, it is clear that all the laws can be stated with much greater precision for particles than for finite bodies, and in fact that the third cannot otherwise be stated in a quite definite form. We must notice at once that neither D'Alembert nor Huyghens have any right to assume *à priori* either (*a*) that the same laws that applied to the large bodies would apply to the little ones, or (*b*) that, if they did, it would be possible to work out the results of applying them to an infinite number of particles with no other conditions imposed than that they form elements of a rigid body. Naturally, unless we could actually prove that this assumption led to the laws that had been found to hold for finite bodies, we should have no justification for making it. But it can easily be shown that *if* the third law of motion applies to any system of bodies the position of the mass-centre of these bodies (defined in the usual way) will either continue to remain unchanged, or will alter with uniform velocity along a straight line relative to axes defined by fixed stars so long as the system is isolated, no matter what special laws there may be determining the mass-accelerations of each particle in terms of functions of the masses, positions, velocities, etc. of the others. But when our system of particles is subject to the condition that the distances between all of them remain constant, we have the case of a rigid body, and so we get back to the first law of motion for finite rigid bodies from the analysis of the motions of which it was first deduced. So that the hypothesis that the mass-points obey the same laws of motion as the finite rigid bodies, is at any rate self-consistent as far as concerns the first law, whilst it allows of much greater rigour of statement.

But we must not stop here, for, by the same procedure, we can show, what was not at all clear before, that the laws of motion, though stated in terms of the translatory motion of finite bodies, are also capable of dealing with their rotations. For, as we have seen, we can reduce rotation of a finite body to the description of closed curves by its particles. But then we can prove that, if these particles obey the third law, and if we assume in addition that the oppositely directed accelerations that they determine in each other are along the line joining them, the sum of the angular momenta of all the particles in an isolated system about any axis fixed relatively to the fixed stars is constant. Similarly, from the other laws of motion and the special laws for the system, we can determine the rotation of any set of particles in the system about any axis. Hence the mere assumption of particles accurately obeying the same laws as were laid down—though imperfectly at best—for the translatory motions of finite bodies make the latter, even in their pure rotations susceptible of mechanical treatment. Hence the object of science is much better attained by stating the laws in terms of particles. The laws that were originally discovered for finite bodies become consequences, and new consequences are discovered for the rotations of such bodies. The only additional assumptions that have had to be made are (*a*) that the opposite accelerations postulated by the third law shall be in the line joining the particles, and (*b*) that the mass of a finite body is the sum of the masses of its particles. With regard to the first point, we saw that the weakness of the third law, as stated for finite bodies, was in the matter of direction, and hence any assumption that we make with regard to direction when we come to deal with particles, where direction first becomes definite, can hardly be said to contradict anything that has been observed of finite bodies. But, indeed, nothing in the interactions of finite bodies would point to the oppositely determined accelerations having any other directions than that along the line of junction. If it made an angle with it, our two magnet poles in water would rotate with ever increasing velocity, which is contrary to all experience. The assumption about mass is necessary to make the first law apply to rigid bodies on the assumption that it applies to their constituent particles. There is no evidence against masses being additive, and, with regard to bodies of finite size, conclusive evidence for it.

I think it should now be clear why we state the laws of motion in terms of particles, although they must have been discovered by considering and analysing the motions of finite bodies. And it is also clear in what sense the statement in terms of particles is justified by those experiences of finite bodies and their motions which alone we can have. Owing to the fact that works on dynamics begin for didactic purposes with the dynamics of the particle, and then pass through D'Alembert's principle to the dynamics of finite bodies, we are often inclined to think that principle merely a mathematical development. But we must remember that the order of knowledge is (1) laws for the translatory motions of finite bodies; (2) the application of these laws to hypothetical particles with the increased precision of statement that this allows; (3) proof that this gives the laws for finite rigid bodies in their translational motions, and that it also enables us to account for the rotations of such bodies without any new assumptions. The last is a real discovery made by the hypothetical method.

The question now arises: Are we to consider the analysis into particles as a real analysis, or is it merely a happy mathematical device? We must remember that, whether we state the laws of motion in terms of velocities and accelerations as we have done, or purely in terms of configurations as Mr Russell does, a statement in terms of particles seems to be equally necessary. Thus we cannot argue off-hand that particles, like accelerations and velocities, are merely mathematical apparatus by which we can indeed start from the existent and get back to the existent, but which themselves have nothing existent corresponding to them. In fact we have to ask the question whether Mr Russell's laws, in so far as they are stated in terms of particles, are any more truly causal, in the sense of dealing with what exists, than the laws stated in terms of velocities and accelerations. Let us then begin by clearly understanding what such a question means on our views. People can perceive bodies, but it is clear that they cannot perceive particles. Hence, whilst bodies can exist as appearances, particles, if they exist at all, must be realities. Now let us consider our views about the nature of the laws of mechanics. As laws taken from appearances and stated in terms of them, we might think that they were laws about appearances, but this is not what we really do think, since we hold that it is quite irrelevant whether anyone is looking on or not to the fact that the laws are obeyed. What we mean by this is that, whilst as stated, they are laws about what we perceive, they are laws about that aspect of it and its changes to which we believe there to correspond a real counterpart. The laws that govern the motions of billiard balls are, as stated, laws about appearances and they were found by observing the motions of perceptible billiard balls. But we believe that there exists something corresponding to the felt round objects and to their visible spatial relations, and that these real relations condition the shapes and distances which, under suitable circumstances, events in the terms that have these real relations, enable us to perceive. Thus our mechanical laws, though stated in terms of appearances, are really about the changes in these real relations to which, when we do have the perceptions, there correspond these perceptible changes from which the laws were reached. We see then that the fact that the particles could not possibly be perceived does not militate against their reality, since, with finite and perceptible bodies, the laws of mechanics only hold of them in so far as they correspond in their spatial relations to a real correlate which exists whether we perceive it or not. But the real reason that makes it unwise to assume that particles are more than a mathematical device is the following. A particle differs entirely from any matter that we ever do perceive in that it has no extension at all. In this respect it is utterly different to assume the reality of particles from what it is to assume the reality of molecules or electrons. A molecule is supposed to be of finite size, though imperceptible. This means for us that it is a real thing with qualities and relations like those which determine our perception of bodies of finite size and definite shape, but that, unlike them, it does not have events in it that can cause us to have a perception of a body of a definite shape and minute size. But, with a particle, all is different. It has no size at all, to speak in terms of appearances, and that means that, if it is real it is not in the least like anything that determines any perception that we have. It has in fact every mark of being a mathematical artifact, and, as such, whilst we cannot deny that it might possibly be real, we have no good reason for supposing that it is so. On this ground I think we may conclude that we were justified in not paying too much attention to Mr Russell's insistence on the statement of the laws of motion in terms of nothing but particles, times and positions; since there is no more reason to suppose that particles really exist, than that velocities and accelerations do so.

The laws of motion then can and must be stated without reference to forces. These are replaced by coefficients which enter the expressions for the amount of acceleration of one particle in terms of the relative positions, velocities, etc., of other particles in the system. We have now a final question to consider. Force is used in statics. Does it here reduce to the same mere name as in dynamics, or must we at length introduce special qualities called forces? As far as the conditions of equilibrium are concerned of course, statics is only a particular case of dynamics, for which the ordinary laws of motion are ample. The only difference that could possibly lead to difficulty is that, whereas dynamics seems to deal wholly with the motions of particles, it is commonly believed that a system may be in equilibrium, in the sense that none of its particles are in motion, and yet 'forces' may be exerted between its parts. Consider, for instance, the case of a bow with the string drawn so that the bow is bent. We hold that all the particles in the bow are at rest (setting aside molecular and electrical theories), and the conditions for this can be determined by the laws of motion. But it is also held that the string and the wood are in a state of tension, and, in fact, that forces are being exerted between one part of the system and the others. Let us try to see precisely what this means. In the first place it means that the shape of the wood is different from what it would be if it did not form part of the system to which it now belongs. It is clear that particles cannot be supposed to change their shape; all that can alter with them is their relative positions. Thus, we must recognise that the same system of particles can have different configurations when it forms part of different systems. But there is nothing in this that has not already been mentioned in the second law of motion. It was known from that that, to determine the positions of the particles in a system at any moment, certain special laws were needed over and above the laws of motions. These laws tell us that, with certain systems of particles, the particles will come to rest in certain relative positions; so that the second law of motion which relates momentary configurations to momentary accelerations will tell us what will be the configuration when there is no acceleration on the part of any of the particles, when once the particular laws for the system under description are known. What happens then is this: We find that the positions of the particles of certain substances are connected with their accelerations according to definite laws when portions of the substance of definite size and shape are made parts of systems, the acceleration produced by which on a particle placed at the point at which the substance is placed we have already determined. We assume that the same laws will be obeyed in any system in which a piece of such a substance forms a part. And, by saying that the piece of the substance in a given system, though in equilibrium, is exerting forces inside the system, we mean that, in the present configuration, it would not be in equilibrium out of that system, but that its particles would be accelerated according to the known laws until a new stable configuration was reached.

Thus once more the alleged 'forces' are nothing but general laws connecting configuration of particles with acceleration in systems of a particular kind. Because these laws hold as a rule wherever a system of particles of the same kind may find itself, we make this law, which is general, into a particular quality of the body, and call it a force. We are more particularly encouraged to do this in statical examples because, if *our* bodies form parts of such a statical system, they themselves are deformed according to certain laws just like other bodies from the shapes and sizes that they have when they are not parts of the system. These deformations, as we saw, are accompanied by a peculiar kind of feeling, and, because this feeling is peculiar, we assume—without justification, as I have tried to show,—that it must have a peculiar cause, viz. a force.

But, although there is no need to assume something special called forces, we must consider how these special laws of matter comport with the statement of all mechanical laws in terms of particles. The special laws that determine for us the form of our function in the second law and the coefficients that it is to contain are supplied by the special sciences like electricity, magnetism, and what is commonly lumped together under the title of 'Properties of Matter.' Whilst one of these special laws—that of gravitation—seems to apply to all kinds of matter at all times and in all states of aggregation, the others do not. The same matter may have different electrical charges at different times, only certain matter is magnetisable, and the constants like rigidity and the coefficients of dilatation and friction vary from one kind and state of matter to another. Now it is true that all these laws, in so far as mechanics can use them, will have to be stated in terms of the relation between the accelerations of particles and the distances, velocities, etc., of other particles together with certain coefficients. But, at the same time, if a given configuration of iron particles is associated with a different acceleration from the same configuration of particles in wood it would seem that we must accept as ultimate the different qualities of material bodies. And, again, if we take the case of charged bodies and find that we can explain their attraction by a law of the form $\frac{ee'}{r^2}$, whilst there is nothing similar in the case of apparently precisely similar bodies which we assert to be uncharged, we seem forced to assume really different states of matter. We can now see what precisely is meant by a mechanical explanation of some physical phenomenon. It does not merely mean a proof that all the motions in this sphere of physics obey the laws of motion; for this is generally believed from the outset. What it means is that, by suitable assumptions, the coefficients in the function required by the second law, which depend on the particular nature of the matter under discussion, can be eliminated. By 'suitable assumptions,' for the present purpose, it is meant that, instead of supposing ultimately different kinds of particles in which acceleration and configuration are connected by the same general form of law but with particular different coefficients, we can explain the phenomena by assuming different invisible configuration and numbers of particles of the same kind connected by laws in which there is no difference of coefficient. When this has been done with the separate 'properties of matter' a mechanical explanation would go on to try and show that all these were special cases of a single law connecting distributions of particles with accelerations. The perceived qualities in which bodies differ must *pari passu* with such explanations be thrust back into different ultimate psychophysiological laws connecting different states of aggregation of the same kind of particles with the clearly different objects that we perceive.

This tendency on the part of the physicist has been bitterly reviled by philosophers, notably by Prof. Ward in his *Naturalism and Agnosticism*. Let us see what attitude we ought to take up towards it.

Since we have seen no reason for supposing that particles as such are real, there is nothing to choose between the statement that a piece of wood and a piece of iron of the same shape and size consist of the same number and arrangement of *particles* with different laws—or, at any rate, with laws that require a different coefficient—and the statement that they consist of different numbers of particles, all alike, obeying the same laws, but giving a different observable effect owing to their differences of number and configuration. The really important question is this: Are we justified in supposing that the causes of the perception of a piece of wood and of a piece of iron of the same size and shape are differently aggregated collections of the same kind of small but finite real bodies differing only by their number and arrangement, and such that when, for purposes of the second law of motion, we make the mathematical analysis into particles, we can assume the same laws connecting configuration with acceleration, and explain the observed differences in terms of the differences of number and configuration? This is the sort of question that is raised by molecular theories, and more especially by the electron theory; and an affirmative answer to it would show that these theories do give information about the real.

Let us suppose for a moment that this could be done; would it have any philosophical importance, and would there be any philosophical objection to it as an account of the real? Our previous results will enable us to answer this question. In the first place we have seen that there is not the least reason, on the assumption that there is a real world events in which cause our perceptions, why, to every difference of quality in the object of our perceptions, there should be one in the cause of the perception. Hence there is no theoretical objection to reducing the ultimate complexity of the real causes of our perceptions below that of the world that we perceive, so long as you are still able to account for the differences in what we perceive. Again, whilst we have seen that there is no intrinsic reason why any of the ordinary sense-qualities should not be real, the causal theory of perception leaves no reason for thinking that they are so. On the other hand, there is some reason for thinking that there is a real counterpart to our objects of spatial perception and more especially to those of touch. Hence, whilst there is no theoretical objection to the reduction of the ultimate differences of the supposed real causes of our perceptions as far as is compatible with the explanation of the differences in their objects, we are positively making fewer debatable assumptions about the real by reducing real qualities and replacing them by laws connecting those that are left and their geometrical relations, as far as is possible. Such procedure does indeed make more assumptions about those qualities and relations that it leaves in the real, but it assumes the reality of fewer ultimate qualities. Now, as the sole ground for assuming real differences of quality is to explain perceived differences of quality, and, as we find that we can best explain the latter by assuming only a few of the former, together with laws about their relations and changes in terms of that real counterpart to the objects of spatial perception which we have been compelled to assume, the procedure of physics does offer us the least objectionable account of that branch of reality with which it treats that is possible. It would be madness to hold that the information that it offers us is certain, when taken as information about the real; it is, and it must always remain, problematic in the extreme. But, with all its imperfections, it remains the best account of the real causes of our perceptions that we can have in the present state of knowledge; and it is only by pursuing the same methods that a still better account can be reached.

It may well be that further experience will show, as it seems to be showing, that the Newtonian laws are only special cases of still more general laws; and that Newtonian mass is not a genuine constant under all conditions. But the more general laws will still be laws about positions and velocities of some extended quality or qualities, and, as such, will be capable of the same sort of defence that I have offered for the traditional mechanical physics; that, although its laws were deduced from experiments made with appearances, they are transcriptions of the laws that hold among the real causes of our perceptions of appearances, and among the relations of these real causes to each other in a real spatial counterpart.

APPENDIX

NOTE ON THE MEASUREMENT OF THE VELOCITY OF LIGHT AND ON THE THEORY OF RELATIVITY

A WORK on philosophy to be in the present fashion must talk about M. Bergson, and a work on space and time must mention the Theory of Relativity. We will confine ourselves to a few words about the more important of these two modish subjects.

It is clear that the equations of electrodynamics, on any theory, raise the same questions about absolute or relative space and time as do the Newtonian laws of motion. For we have the old difficulty that the laws are supposed to be the results of empirical observation, and to be stated in terms of absolute space and time. They contain moreover a certain constant c which has the dimensions of a velocity, and is identified with the velocity of light, or any other electromagnetic disturbance, *in vacuo.* So the question arises whether this is an absolute or a relative velocity.

If electrodynamics merely raises the same problems as ordinary mechanics, and provides no material for a further discussion, it will not be worth while to trouble about it. There will be nothing to discuss except the interesting question whether the laws of electrodynamics hold for the same axes (those defined by the fixed stars) as the laws of motion, and this can be left to the physicist. But the velocity of light or any electromagnetic disturbance is a somewhat different thing from that of a piece of matter; and electrodynamic considerations have given rise to the Theory of Relativity which is believed by some of its supporters (notably by the late distinguished mathematician Minkowski[1], who did so much for its theoretical development) to necessitate entirely new views about space and time. It will therefore be worth our while to discuss shortly the assumptions involved in measuring the velocity of light, and the Theory of Relativity.

Suppose you know the time at which a disturbance leaves a body A, and the time at which it reaches another body B, and the distance between the two, you have necessary—though not perhaps sufficient—factors for determining the velocity of propagation of the disturbance. But, in practice, the determination even of these factors involves certain assumptions which are not commonly made explicit. It is one of the greatest and least equivocal of the services of the Theory of Relativity to have called attention to this fact. Let us leave that theory out of the question for the present and consider for ourselves the problems involved in the determination of these three factors.

If you want to measure the velocity of a body it is a truism that you must be able to identify it. It must be the same body that leaves one of your fixed points at one moment and reaches the other at the second moment. Similarly, in measuring the velocity of light, we want to deal with the same disturbance throughout. This identification is made in a simple way in Fizeau's method of using a toothed wheel to cover and uncover a source of light successively. The disturbance that we have to deal with is that emitted by the source during the time that the opening is faced by a definite one of the gaps in the rotating toothed wheel. This disturbance passes to a distant mirror, and is reflected back to the toothed wheel. If the wheel be rotating below a certain speed the disturbance will come back to find a part of the gap from which it came still open, and an image of the source will be seen. But, if the speed be increased, a velocity will be reached such that, when the light that has passed through one gap in the wheel returns from reflexion, it is faced by the tooth which forms one side of this gap; it cannot pass through and the image disappears. If the speed be further increased it returns to find the next gap opposite the source; it passes through; and the image is restored. Thus, if you first increase the speed till the image vanishes, and then increase it till it reappears, you know that the image that you see is due to the light that passed through the gap next before the one through which it is now returning. There is not absolute identification, but you know that you are dealing with some part of the light emitted during the very short time that the opening is faced by any given gap; and that is enough. It is because there seem to be no bodies that are opaque to gravitation, as so many are to light, that we cannot measure the velocity of gravitation or prove that it has one at all; for we cannot identify the 'gravitational disturbance' emitted by any body at a given moment with that received elsewhere at another moment.

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[1] *Raum und Zeit.* For a full exposition of the Theory of Relativity see *Das Relativitätsprinzip* by M. Laue, and, for a more elementary account, *Das Relativitätsprinzip der Elektrodynamik* by G. O. Berg. The best discussion of the subject in English is the very able article by Prof. Huntington, *Phil. Mag.* April, 1912. Two interesting pamphlets, called *Optical Geometry of Motion* and *A Theory of Time and Space*, by Mr A. A. Robb, may also be mentioned.

So much for identification. I will continue to consider the Fizeau method, because, although it is not in practice so good as that of Fizeau and Foucault, which uses a rotating mirror, it is more straightforward and therefore better adapted as an example for a discussion of general principles. Let us turn then to the two positions involved in the measurement of a velocity. In this method the two positions at which we actually observe are indeed identical points on the same instrument. But here, as everywhere else, you must measure a distance, and so deal with two different points in order to measure a velocity. The two points are of course the gap and the centre of the mirror. In terrestrial experiments the distance of these would of course be measured by means of rigid bodies, whether in the gross form of meter scales or in the refinements of micrometers. But many long distances have to be measured by means of light signals, and, again, our most accurate measures of very small differences of length are by means of the shifting of interference bands. Both these methods involve a knowledge of the velocity of light, and would therefore be inadmissible in experiments to prove that light has a velocity or to determine its particular magnitude. But the presuppositions of ordinary straightforward measurement with scales have not always been made explicit, and it is a merit of the Theory of Relativity to call attention to them. They involve, in fact, time as well as space.

It has always been recognised that measurement by superposition involves at least one assumption which cannot well be verified, and that is the constancy of length of the measure. The difficulty is of this nature. I use a scale to compare two lengths because I do not trust my immediate judgments of comparison under certain circumstances. The circumstances are the following. (i) Even if I can see two lengths at the same time, yet, unless I can put them side by side and notice whether there is any overlap, I cannot trust my immediate judgments of comparison. And (ii) the case is worse when I cannot perceive the two objects at the same time in any position. For I then have to compare what I see with what I remember, and memory in such matters is very untrustworthy. By the use of a portable scale I obviate the first difficulty altogether; but I only obviate the second on the assumption that my scale remains of the same length when I carry it about. Now my judgment on this point rests on memory, to correct the untrustworthiness of which we used a scale at all. This is to put the difficulty in its most striking way; but actually there are modifications to be made. (i) We happen to have the power of actually observing motion, if it be within certain limits of speed, as a special object. Now we can keep our scale under view while we are carrying it about, and, since we do not observe this phenomenon, we can be sure that, if it be changing in length, it must be either very quickly or very slowly. We can further be sure that it is not changing very quickly, for, if it were, even our imperfect comparisons based on memory would soon indicate a change in length to us. Hence we can at least conclude that, if our scale does change in length, it does so either very slowly or, at any rate, by a very small total amount. (ii) The untrustworthiness of judgments of comparison based on memory increases very quickly with the time that elapses between the perception of what is now remembered and the present memory of it. If we compare two distant lengths it is not in our power to reduce this period, which depends on their distance apart, beyond a certain amount; but it is possible to keep the scale under practically continuous observation. If I want to compare a length at Cambridge with a length in London by memory not less than 1½ hours can elapse between the two observations; but, if I carry a metre scale with me, I can (at the risk of alarming my fellow-passengers) contemplate it during the whole journey from Cambridge to King's Cross. I am well aware of course that this is not conclusive. L_{n+1} may look as long as L_n, and L_n as L_{n-1}, and so on; and yet actually there may be such a shortening that, if I could directly compare L_{n-1} and L_{n+1}, I should at once see that their length differed. Still I am at any rate less liable to error than if I trusted immediately to memory. For, be it noted, the length of the lapse, which decreases the trustworthiness of direct comparisons by memory of things in widely different places, may very well be a positive help here. For, if the slow change goes on continually in one direction, I shall surely be able to detect it even by memory sooner or later. If A and B, the two distant bodies, were nearly equal then memory is a bad guide; but, if A be the length of my metre scale when I left Cambridge, and B its length at King's Cross, and B is actually considerably shorter than A, though at any two observations made near together on the journey the lengths appeared the same, then the defect of memory due to lapse of time is being compensated by the actual growth of difference between what I observed at Cambridge and what I observe at King's Cross; for comparison is always more trustworthy for great differences than for small ones. (iii) Finally, there is a more general argument in favour of assuming the constancy of length of a scale when there is no special reason to doubt it. Suppose that we have done as we should do in all accurate physical measurement, and eliminated the effects of those causes which we *know* to influence the length of material bodies. We are left with unknown possible physical causes, and with the possibility that the mere fact of motion influences length. Now the unknown physical causes might depend on the path pursued in carrying the scale from A to B. These could be neglected if we found that our comparison of the lengths of A and B by the scale were the same no matter what path we pursued[1]. The main exceptions to this would be (1) if the change of length of the scale depended on bodies so distant that any difference of path that we could make would be negligible in regard to them, or (2) if the change were due to bodies within the earth, since we could not follow paths both in and out of the earth to see whether there were any difference. (1) may fairly be neglected, on the ground that in all science we are obliged to assume that the influence of bodies decreases with their distance, and that we know (as far as we can be said to know anything in the empirical world) that physical forces and disturbances, like light and sound and gravitation, diminish rapidly in intensity with distance. I do not know how (2) could be eliminated. Another possibility is that the change of length is not due to the path pursued, but depends on circumstances which are only operative quite near the bodies to be measured. Here we can only say that, since, by hypothesis, the change is sudden, we might fairly expect it to be observable.

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[1] Of course this is only true if you are using a scale to measure and compare two distant bodies; if you are measuring the distance between two bodies this method of elimination is no longer possible because you must push the scale along the line joining them.

Lastly, the question of the possibility of length being dependent on velocity needs a moment's consideration. It will meet us again in the Theory of Relativity. We must begin by noticing that it is a suggestion that has a much more definite meaning on the theory of absolute than on that of relative motion. It is perfectly possible that, if there be absolute space and time, there may be laws connecting the lengths of bodies with their absolute velocities. In Lorentz's[1] and Fitzgerald's theories of electromagnetic phenomena in moving bodies it is found necessary to suppose that bodies alter their linear dimensions with their velocities along the direction of motion but not at right angles to it. But the velocities contemplated are absolute ones, or, at any rate, relative to the ether only. If it were suggested that there is only motion relative to other bodies, and yet that length depends on velocity, we should have a strange position; for every body in the universe has relative motions of the most various magnitudes and in the most various directions at the same time. If I walk across the room my metre scale has a velocity relative to me, and, at the same time, it has velocities relative to all the white ants who are moving about in South Africa. It were surely very wild to suppose that these relative velocities—which are just as real as any others—genuinely condition the length of bodies, unless we have the strongest grounds for doing so. But it will be best to defer the further discussion of this theory until we have seen why it is supposed to be needful to assume it.

The upshot of this discussion is that here, as in the choice of axes for mechanics, and the determination of equality of times, we start with a belief in the general trustworthiness of our judgments of comparison, then proceed to criticise and modify on the basis of what seems probable from our general knowledge of the 'make' of the world, and finally take as equal those qualities which at once seem equal to our senses as corrected by the best means at our disposal, and, by being taken as equal, enable us to state verifiable general laws about the real world in the simplest possible terms. Here, as everywhere, there is always the possibility of our being wrong—and I would add the possibility of *theoretically* incorrigible mistakes; but, until we are proved wrong, we shall act reasonably in believing ourselves to be right. What I would particularly deprecate is that perverted kind of idealism of which the philosophic works of the late M. Poincaré are full, which holds that, when we can imagine a physical law which would make our measurements in error and yet would not allow us ever to detect the mistake, we must say that there is nothing absolute to measure and (what certainly follows) that there can be no right or wrong in the matter. We may surely recognise, as a matter of theory, that we may be liable to genuine errors which we shall never discover; but, having made this acknowledgement, we need never trouble further about them.

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[1] Lorentz, *The Theory of Electrons.*

The difficulty which I have been discussing about measurement is, as I said, an old one, but it is worthy of consideration. There remains, however, another connexion between the measurement of lengths and time, the implications of which have only lately been recognised. This connexion is very relevant to the velocity of light and the Theory of Relativity. If there be absolute space the distances between points in it are not functions of time but are eternal relations. But, whether there be absolute space or not, it is certain that we cannot measure directly the distances between its points, but must confine ourselves to the distances[1] between bodies in space. Now the distances between bodies are not eternal but are liable to change. What we want to know is the distance between two bodies at a definite moment, for this alone is certainly definite. Hence time enters essentially into the *measurement* of spatial magnitudes, though not, so far as I can see, into the *notion* of space, as Minkowski would have us believe.

It is evident that no process of measurement can *directly* give us the distance between two bodies at any definite moment, though, under certain circumstances, the results of our measurement may as a matter of fact coincide numerically with the value of the distance at some definite moment. Unless my measuring-rod exactly fits the distance to be measured, it is clear that the moment at which, in the course of measuring, one end of the rod reaches the body *B* cannot be the same as that at which the other end is at *A*, but will be later. But what we want to know is the distance between *A* at one moment and *B* at the same moment. We might feel tempted to say that, if *A* and *B* be relatively at rest, then the number of times that I lay the measuring-rod down in passing from *A* to *B* will give their distance at any moment during the period for which they remain relatively at rest. But this forgets an essential factor. The measuring-rod itself is a material object, and has to be laid on something

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[1] I do not attempt here to discuss what precisely *the* distance between. two bodies can mean. But I mention the matter as being one full of difficulties that have hardly been touched as yet.

—call it a third body X. If there be no relative motion between A, B, and X, and none between X and the rod at the moments when the positions of the latter's end-points are being marked on X, what we measure will give the distance at any moment between A and B. If, on the other hand, whilst the same conditions are fulfilled, as between A and B on the one hand, and X and the rod on the other, but A and B are moving relatively to X, what we measure will not be the distance between A and B at any moment. It will, in both cases, be a measure of the distance between the point on X with which A coincided when we began to measure and the point on X with which B coincided when we ceased to measure, but this will no longer coincide with the distance between A and B either at the moment when we began or at that when we ceased measuring. If we forget this consideration, and if A and B both move in the direction $\overrightarrow{AB}$ with the velocity v relative to X, whilst the rod when in position for reading is at rest relative to X, it is clear that we shall reach different results according as we measure from A to B or from B to A. If we measure from A to B the distance from where A was when we started to where B is when we end is greater than that between A and B at any moment, and the difference will depend on how fast we measure. If we measure from B to A our measured distance will be too short. Finally, if v be greater than the rate at which we measure, we shall never overtake B, and shall therefore judge that the distance between A and B is infinite as compared with the length of our rod. If, on the other hand, we measure from B to A, it may be that A will have reached the end of the rod that was at B before we move the rod, and then we shall conclude that there is no distance between *A* and *B*.

All this sounds silly enough when we put it in terms of rods. We say that we should see *A* moving up the rod as we measured from *A* to *B*, or *B* moving away from it as we measured from *B* to *A*; and that, anyhow, we could tell that something was wrong, because, if the measured distance from *A* to *B* differed from that from *B* to *A*, we should know at once that we could not be dealing with the distances at the same moment, since distance at the same moment is symmetrical. This is doubtless true, but it would not help us if the velocities with which we had to deal were very small; and it is only because we do not use rods for measurement in the cases where errors of this kind are at once important and difficult to detect that the problem does not arise with measuring-rods. But it arises acutely when we deal with the velocity of light, and this in two ways. In the first place, it enters into the actual determination of the velocity of light, and, further, it is involved in all measurements of distance made by means of that velocity when it is supposed to be known.

Let us return once more to our Fizeau experiment to illustrate these statements. We take as the distance that the light has travelled twice the measured length between the wheel and the mirror. We have good grounds for holding that these are relatively at rest to each other and also that both are relatively at rest to the earth on which we lay our scale. Hence the distance measured is actually that between the two at any moment. But did the light travel this distance when it left the gap and reached the mirror, and does it travel the same distance when it leaves the mirror and returns to the gap? We are likely to say that the actual distance travelled by the light is not from where the gap is when it leaves it to where the mirror is at that moment; nor from where the gap was when the light reached the mirror to where the mirror is then; but that it is the distance between where the gap was when the light left it and where the mirror is when it reaches it. But here the consistent relativist is on very slippery ground, and, if he thinks that he is merely using the language of common-sense, he must remember that common-sense is not relativistic. By the 'place where A was' the relativist must of course mean a place defined by distances from some material axes. Hence the 'place where A was' will depend on your axes. If you take axes fixed in the earth the place where the gap was when the light left it is the same as the place where it is when the light returns to it, and the place where the mirror was when the light left the gap is the same as the place where it is when the light reaches it. For there is no relative motion between the instrument and the earth. If, on the other hand, we take the fixed stars to define our axes the place where the gap was when the light left it will no longer be the same as the place where the gap is when the light returns to it; and naturally the same remarks apply to the mirror. This is of course because of the rotation of the earth relative to the fixed stars. What we have to be careful to remember is that the relativist has no right to the phrase '*the* place where A is' because he must mean its place with respect to some definite set of material axes, and, according as he chooses a set X or a set Y, the place where A was at t_1 may be the same as or different from the place where it is at t_2. What the relativist must talk about is always 'the place of A relative to X' and 'the place of A relative to Y'; there is no inconsistency in one of these changing and the other being unaltered in an interval τ. Common-sense on the other hand can talk of '*the* place of A' at a given moment, because it means its place in absolute space. It follows that the relativist has no right to talk of *the* distance travelled by the light. The light left the gap, reached the mirror, and returned to the gap. Relative to the earth the distance travelled is twice that measured between the gap and the mirror; relative to the fixed stars it is different from this. If you object that, after all, there must be some definite distance that the light has travelled independent of your choice of axes, I can only answer that, if you think so, you must drop your theory of relative space. No doubt the distance between two bodies at any moment is independent of choice of axes; but, when the distance required is that from where A was at one moment to where B is at another moment, it is no longer the distance between A and B at any moment that you are measuring but the distance between two hypothetical pieces of matter A' and B', rigidly attached to your particular system of axes, and such that A coincided with A' at the first moment and B with B' at the second moment.

We might expect then that, if we divide twice the measured distance by the time elapsed between leaving the first gap and re-entering the next, we shall get the velocity of light relative to the earth, but not relative to the fixed stars. (This of course assumes that the velocity of light is constant and that no time is taken up by reflexion at the mirror. These assumptions I do not propose to discuss.) We can now pass to the celebrated Michelsen-Morley experiment. Stated in its simplest terms the experiment comes to this. Light from a source fixed in the earth is sent out in a direction tangential to the earth's orbit. It is then partially reflected and partially transmitted by a mirror fixed at a point *P* in this line. One part continues in its old direction, and the other goes at right angles to it and therefore to the tangent to the earth's orbit. Both parts are reflected back along their respective paths by mirrors placed normally to them at the same measured distances from *P*. Now, owing to the motion of the earth in its orbit, the light that has travelled straight on in the tangent and back again has described a slightly longer path relative to the fixed stars than that which was deflected at right angles. Since the velocity relative to the fixed stars is constant in all directions the two disturbances that meet after the reflexions will not have started from the common source at exactly the same time. They will therefore interfere. If we turn the whole apparatus through a right angle in its own plane the interference bands ought to have shifted, and the amount of shifting should enable us to compare the velocity of the earth in its orbit relative to the fixed stars with that of light. Michelsen and Morley's apparatus was quite delicate enough to detect changes of this order, but none were detected.

Another and an equivalent way of stating the result is as follows. If the velocity of light relative to the fixed stars be the same in all directions, whilst the earth has a velocity relative to them in a certain direction and none at right angles to it, we should expect the velocity of light relative to the earth to be different for paths along and at right angles to its direction of motion. Hence, if the paths on the earth are equal, light ought to take different times to traverse them, and so there ought to be this interference phenomenon which actually is not observed.

It is the non-observability of such effects which is the motive of the Theory of Relativity. We can already see that one of the alleged paradoxes that flow from this Theory, viz. that if A has velocity u relative to B and B has velocity v relative to C in the same direction then the velocity of A relative to C is *not* $u+v$, is realised in the experiment; for, if we call c the velocity of light relative to the fixed stars and u that of the earth relative to them, it would follow from this law that the velocity of light relative to the earth would be $c-u$ in the direction of the earth's motion and c at right angles to it, whereas both velocities are found to be equal. But before we actually pass to the Theory itself there are two points worth mentioning. (i) In the Michelsen-Morley experiment we are sending out light from a source which is in motion relative to the fixed stars. If we supposed light to consist of particles obeying the ordinary laws of mechanics and shot off from the emitting body by impulsive forces which are the same in all directions, then those particles which travelled parallel to the earth's direction of motion would start with the common velocity due to the impulse + the velocity that they already had as parts of a body moving relative to the fixed stars. Those which were shot off at right angles to the earth's motion would only have the velocity due to the impulse in that direction, though they would of course drift with the velocity of the earth in the direction of its motion. The result would be that the velocity of light from a source moving relatively to the fixed stars would not be the same relative to the fixed stars in all directions, and this might explain the negative result of the experiment without resort to anything more complicated. We should further, I take it, have to suppose that on passing through the glass plate at P the direct ray would be retarded and that this retardation would be permanent. On the wave-theory it is of course retarded, but only for as long as it is travelling in the glass. It is worth while to mention this alternative theory, but hardly to work it out, since all the arguments by which physicists have persuaded themselves that the undulatory theory is to be preferred to the emission theory are against it. (ii) The purely physical theory to account for the result of the Michelsen-Morley experiment is that of Lorentz and Fitzgerald that bodies in motion relative to the fixed stars undergo a contraction in the direction of their motion, which depends on their velocity, but none at right angles to it. This contraction cannot be detected, because it affects the measuring scale too; so that lengths measured on the earth in various directions, which appear equal, are not really so. It can be shown that, if a length which is l when there is no motion relative to the fixed stars becomes $l\sqrt{1-\frac{v^2}{c^2}}$ when it moves with velocity v along its own direction relative to the stars, but remains unchanged if moved at right angles to itself, the observed results can be predicted. (The letter c stands here for the velocity of light relative to the fixed stars.) Of course Lorentz does not mention the fixed stars but talks about motion relative to the ether, which he supposes to be fixed in absolute space. Since you can no more determine velocities or positions relative to the ether than to absolute space, and since the former is supposed to be fixed in the latter, I see no advantage in retaining both conceptions. If either is to be retained absolute space has much the better claim. But, in any case, the empirical laws must have been discovered by reference to actual bodies, and these are here, as in mechanics, ultimately the fixed stars, or a system of co-ordinates so imagined as to make a form of statement of the laws which is approximately true in terms of motions relative to the fixed stars completely true in terms of motions with respect to it. The objection to the Lorentz-Fitzgerald theory as it stands is its apparent arbitrariness. I do not think that it is a legitimate objection to say with Poincaré that the contraction is postulated *ad hoc.* Of course it is postulated *ad hoc*; so is everything in physics that cannot be directly observed. Nor is it relevant to say that there is a very great difference between postulating something to account for what you do observe and postulating something to account for your observing nothing. To observe nothing where, on theories which have so far accounted wonderfully well for all the phenomena, you ought to observe something, is a very positive fact; it shows that your theories are either wrong, or, at any rate, incomplete; and, in as far as they have been so successful over so large a field, it is reasonable, not to reject them entirely and start afresh, but to seek for some supplementary hypothesis that shall account for their present failure. My objection to the theory is that, when you leave out of account ether and absolute space, it seems strange and improbable. Let us grant at once that this is no conclusive reason against it, and that no causal law that you can possibly suggest is *à priori* impossible, and let us then consider what is involved in this suggested law.

If we are to be consistent relativists we must say that length, on this theory, depends on motion relative to the fixed stars. We shall then probably be asked why motion relative to the fixed stars should be connected with length rather than motions relative to (say) the most hard-roed codfish in the Atlantic, since one set has no prerogative of reality over the other. But our discussions about dynamics will enable us to dispose of this question with something more than the general answer that you cannot prescribe *à priori* what in nature is going to be relevant to what. The answer in fact is that length will be as much connected with motions relative to the codfish, or any other body of reference, as with motions relative to the fixed stars. All that we have to add is that, if you refer the motions of all bodies to the fixed stars, the law connecting their lengths with their velocities will take the simple form given by Lorentz and Fitzgerald, whilst, if you refer their motions to axes fixed in the codfish, the law (though equivalent) will be of excessive complication. There is nothing arbitrary in an objectionable sense in the Lorentz-Fitzgerald law when stated in terms of the fixed stars; it gives the fixed stars no prerogative in nature but only a prerogative in human calculations. Leibniz's God, with his passion for complicated mathematics, would probably prefer the codfish as a body of reference, and would commit no error in doing so. You may, if you choose, make an objection somewhat like Mr Russell's question whether the fixed stars on the relativist view are subject to Newtonian forces. You can say: At the same moment the fixed stars cannot have two different sizes; when you refer motions to them they will not be shortened, since v in the Lorentz-Fitzgerald formula will vanish. But, when you refer them and other bodies to the codfish, they will have a definite velocity, and they will be shortened. All this, however, is sheer confusion. If the Lorentz-Fitzgerald formula be strictly true for the bodies in the world when referred to the fixed stars then the equivalent law when you refer all bodies, including the fixed stars, to the codfish must be such that the absolute size of each body at the same moment is the same.

So far then the Lorentz-Fitzgerald law, though stated by its authors in terms of absolute motion or (what is equivalent) of motion relative to a fixed ether, furnishes, even if true, no more ground for a belief in absolute space or a fixed ether than do the laws of mechanics when similarly stated. But where people feel a difficulty is here. Is it possible, they will ask, that an intrinsic quality of a body, like its length, can be causally connected with a mere change in its external relations like its relative motions? We do not, I must repeat, in ordinary life believe for an instant that motion is *merely* a change of distance either between two bodies or between bodies and points of absolute space; it is only by a great effort that we can bring ourselves to keep this view firmly before the mind in all its bearings, and the fallacies with which discussions on absolute and relative motion bristle (some of which I can hardly expect to have been fortunate enough to avoid) show that, with the slightest relaxation of attention, we fall back into the notions of common-sense. We regard the motion of each body as a quality of it as much as its size, and, whilst we grant that this quality is connected with changes of spatial relation between bodies, and, if we believe in absolute space, between them and it, I doubt whether even the acceptance of absolute space, when coupled with the assertion that the motion of a body is *nothing but* the change of its relation to points of absolute space, gives what common-sense thinks it means by motion. Now common-sense, holding this belief that the motion of a body is something more than the change of its spatial relations, finds no particular difficulty in thinking that this quality may be connected with the other quality of the body called length. It is surprising, like the properties of Radium, because unfamiliar; but that is the worst that can be said of it. But, if you ask common-sense to drop the idea that you can talk of the motion of a single body as a quality of it, and to confine itself to the change of distance between it and other bodies—a perfectly reciprocal change—and then tell it that this is causally connected with the size of the body, it will tell you that it is inconceivable. Nor would it be much comforted if you told it that the size was connected with the rate at which its distance was changing with respect to points of absolute space. It will say to the first suggestion: I am ready to believe that if I walk across the room my height alters, because the motion and the height are both qualities of me; but I cannot believe that if you walk across the room the mere change of distance will alter my height, for my height is a quality of me and the change of distance a mere alteration in a reciprocal relation between us. I am even ready to believe that *your* motion, if you will let me take it as a quality of you, affects *my* height, for I know of plenty of cases of transeunt causation; but further than this I cannot go. Put into abstract form, the position of common-sense is that, if a reciprocal relation between two terms involves no qualities in those terms except that of standing in this relation, then a change in this relation cannot be causally connected with changes in those qualities which can be seen not to be dependent for their existence on the existence of other terms. Size is such a quality; common-sense is firmly convinced that, if there were only one body in the universe, it would have some definite size and shape at each moment; and, if you answer that size depends on relations, you simply commit the fallacy of confusing magnitude with the possibility of measuring magnitude. The same difficulty is not felt with mechanics, because here we have causal relations between changes of distance, not between them and qualities of terms.

Frankly I am not prepared to pronounce on this dogma. When I reflect on it I do not see that it is self-evident; but one must attach a certain weight to general principles which many people find evident, unless one can either show that they are false, or point to causes which have actually produced the belief but include among them beliefs incompatible with it. The dogma remains the only serious objection to the Lorentz-Fitzgerald theory, assuming that the latter accounts for the facts. But, if we could show that there was no need for any special physical assumption, that the negative result of the Michelsen-Morley and numerous other experiments depends on assumptions involved in all measurement, which are commonly overlooked and only here begin to be relevant, we should gain a double advantage. We should no longer go against what may be an *à priori* law, and what, at any rate, renders such a theory as that of Lorentz intrinsically improbable; and we should not have to alter or supplement our old, and, so far, successful theory of electrodynamics, but merely to develop the consequences of something in it that had been overlooked. The Theory of Relativity claims these advantages, and we will now consider it.

Let us consider two systems which are in relative motion to each other, and suppose that the people on them are trying to find the magnitude of some physical quantity like the velocity of light. It is clear that, if we call S_1 and S_2 the two systems, the question: Is the velocity of light relative to S_1 the same as or different from its velocity relative to S_2? has no very definite meaning till we are sure that the people on both systems are using the same units of length and of time, or, at any rate, know the relations between their respective units. Of course, *in nature*, light has a certain velocity relative to each system, and these velocities are either the same or different, whatever may be people's measures of them. But all that we can know is whether the numerical values found by the two sets of observers are the same; and it is clear that their sameness or difference will only allow us to infer to the sameness or difference of physical velocity if they are using the same units. If, again, they think they are using the same units, but their judgments on this point are in fault, sameness of measure will not mean sameness of magnitude measured, and difference of measure need not mean difference of magnitude measured. These points seem obvious; but, as most difficulties here spring from neglecting to make the obvious explicit, I mention them. Let us first consider their judgments about time. It is clear that they must not merely agree about their units of time, but must also agree as to when a physical process is uniform. The only thing they can do is to agree to take some definite physical process, which they can both observe, as uniform, and to standardise their measuring instruments by means of this. As we have already pointed out, there is an element of convention + an element of immediate judgment involved in taking a certain process as uniform. We begin with immediate judgments of comparison, and end by defining as uniform some process which (*a*) appears uniform to our judgments of immediate comparison, and (*b*) by being assumed uniform, simplifies as far as possible the statement of laws that are found to be true. We can suppose that each set of people has gone through this process for themselves independently of each other and that each has decided that the velocity of light relative to his system is uniform. This does not imply that each has found *the same numerical value* for it, for that would involve that they have agreed about units of length and time[1]. This being assumed, we can suppose that they both use the same methods of regulating their clocks so that they shall go uniformly. The method adopted by both will be as follows: When a clock on S_1 marks t_1 a light signal is sent out to some other point in the direction of relative motion and reflected back. It returns when the clock marks t_2. Another signal is sent out to the same point when the clock marks t_3 and returns when it marks t_4. If you always find that $t_2 - t_1 = t_4 - t_3$ the clock will be said to be going uniformly. The people on S_2 will regulate *their* clocks in the same way.

Now we can suppose clocks dotted about S_1 and S_2 wherever we please, and the next point that you must agree about is in your test for whether all the clocks in a system are going at the *same* rate, it being granted that each goes at a *uniform* rate. Here two methods present themselves. If you can carry the clocks about you have only to bring them up to the clock that you are going to take as standard on S_1, and see whether, while this clock's hands pass from t_1 to t_2, those of all the others pass through the same angle. If so you can further completely synchronise the clocks by seeing that all mark t_1 together. Assuming that the rates of your clocks will not be altered by carrying them about they can now be put back in their places on S_1 and all the clocks on that system will be running at the same uniform rate and having the same zero. The people on S_2 can synchronise their clocks with their standard one in the same way. But, if you cannot synchronise your clocks in this way (and it is very unlikely that you could do so for all the clocks of a system), another plan must be adopted by the people on each system. Let the standard clock on S_1 send out a light signal to a point P on S_1 when it marks t_1. Let this reach P when P's clock marks t_2. Let the standard clock send out a second signal when it marks t_3, and let this reach P when P's clock marks t_4. Then if $t_3 - t_1 = t_4 - t_2$ we say that the clock at P is going at the same rate as the standard clock. It only remains to set the zeros of the other clocks on S_1 in accord with the standard one. If the standard one sends out a signal when it marks t_1, and this is received at P when P's clock marks t_2 and at once reflected back to the standard clock, reaching it when it marks t_3, we say that the zeros of the two clocks agree, provided that $t_2 = \frac{1}{2}(t_1 + t_3)$. The people on S_2 are supposed to set their clocks in the same way.

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[1] In what follows about the regulation of clocks and the comparison of rates and zeros I am very much indebted to Prof. Huntington's article already quoted.

So far we have assumed that both people have concluded that the velocity of light is uniform independently of each other. It is clear that, if this way of setting clocks be reasonable at all, it is *as* reasonable for the people on S_1 as for those on S_2. And the supposed uniformity[1] of the velocity of light makes it reasonable for both. Let us now consider their measures of length. Granted that *each* holds that light travels uniformly relative to him, and remembering that they cannot possibly compare the velocities of light which each finds relative to his own system with each other until they have agreed on their units of length and time, we see that there must be an element of convention in the answer to the question whether the velocity of light relative to both is the same or different. It is clear that they can so choose their respective units that the *numerical values* of the two velocities will be the same, and, if they further have grounds for thinking that their units are the same under these circumstances, they will naturally conclude that the actual velocities of which their measures are equal are equal in nature. Now the people on S_1 are at liberty to define their units in such a way that if a light signal leaves a point P when P's clock marks t_1 and reaches a point Q when Q's clock marks t_2 the measure of the distance PQ shall be $c(t_2 - t_1)$. c is a mere number which they can choose as they like, and whatever they choose will be the numerical value of the velocity of light relative to them in their units. Now, if the people on S_2 have synchronised their clocks in the same way as those on S_1, and have decided to set out a coordinate system in the same way by light signals, their choice of units of space and time are still open to them. They can make their standard clock go at any rate they like compared with that of S_1 and they can choose any unit they like for distance. Since, *until* they have chosen their units, they cannot say whether the velocity of light relative to them is the same as or different from that of light relative to S_1, they are at liberty to choose their units so that the two velocities shall have the same measure.

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[1] Prof. Huntington does not mention this point, which seems to me important. Unless they both assumed this it would be an unreasonable way of setting clocks for the ones who did not assume it.

This will not of course supply them with any reason for supposing that the two velocities are equal in fact, unless they have grounds for believing that their units of time and space are the same multiples of S_1's units of time and space respectively. Thus the fact that the measured velocity is the same relative to the two systems depends on a convention about units. Still the question whether the velocities themselves, as distinct from the measures of them, are equal is not a matter of convention; it depends on whether equal measures of length and time, as determined on these conventions, really correspond to equal lengths and times in nature.

Let us suppose that the people on S_1 reckon that the standard clock on S_2 is going r times as fast as theirs. Their method of judgment is as follows. They find that when they observe this clock to mark t_1' the clock on their system which is opposite to it marks t_1, that when they observe it to mark t_2' the clock on their system opposite to it marks t_2, and that $\frac{t_2' - t_1'}{t_2 - t_1} = r$. Let us further suppose that the people on S_1 reckon that S_2 is moving with a velocity u relative to them along their x axis. Then it is easy to show, and it is shown by Prof. Huntington, that, if the clocks be synchronised and the coordinates set out by light signals in accordance with the rules laid down, the following relations will hold between the coordinates and clock-readings of the two systems. Let the standard clocks be at the respective origins of the two systems, and, when these origins are opposite each other, let the clocks mark O. Further let the common direction of relative velocity be taken as the x-axis in both. Then, if x, y, z be the coordinates of a point on S_1 and the clock there reads t_1, whilst x', y', z' are the coordinates of the point on S_2 opposite to this and t_2' its clock reading, the following equations hold:

$$x' = l\kappa(x - ut), \quad y' = ly, \quad z' = lz$$

$$t' = l\kappa\left(t - \frac{u}{c^2}x\right)$$

$$\kappa = \frac{1}{\sqrt{1 - \frac{u^2}{c^2}}}, \quad l = \frac{r}{\sqrt{1 - \frac{u^2}{c^2}}}.$$

These are the usual equations of the Theory of Relativity. It can be shown that, with our conventions, the numerical value of the velocity of S_1 relative to S_2, as measured by people on S_2, is $-u$, and that the rate of the standard clock on S_1, as observed from S_2, is $\frac{1}{\kappa^2 r}$. It will then be seen that the equations are—as they ought to be, seeing we are dealing simply with relative motion between the two systems—completely reciprocal; *i.e.* we have

$$x = \kappa l' (x' - u't') \text{ etc.}$$

A further very important result is that the numerical value of the velocity of light from a source in uniform motion relative to *the two* systems is found to be the same, viz. c by observers on each relative to his own system. The fact that this follows from simple and obvious considerations about measurement on Prof. Huntington's method seems to me to be its great advantage over that of Einstein. Einstein[1] takes this by no means obvious result as an independent postulate, and it is from this that he actually obtains the transformation equations by mathematical reasoning. I say that he takes it as independent; I should say that he ought to do so. Actually he seems to think

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[1] *Jahrbuch d. Radioaktivität u. Elektronik*, Jan. 1908.

(wrongly, as is well brought out by Mr Norman Campbell[1]) that it is deducible from the axiom that the velocity of light relative to a system from a source *fixed* in it and measured by people on it will be independent of the relative motion of the system to others[2].

The Lorentz-Fitzgerald formula follows at once from these equations, and therefore—when measurements are made by light signals, at any rate—from plausible conventions about measurement, and from these alone. Let P and Q be two points on the x-axis of S_1, and let their coordinates be x_p and x_q. Then the people on S_1 will call the measure of PQ $x_q - x_p$. The measure which people on S_2 will give of their distance will be the measure that they give of the distance between two points P' and Q' on S_2, such that, at the same moment by the clocks on S_2 (say t'), P' is opposite to P and Q' to Q. Then we have:

$$x_p' = \kappa l\,(x_p - ut_p),$$

and

$$x_q' = \kappa l\,(x_q - ut_q),$$

when t_p and t_q are the readings of the clocks on S_1 at P when P is opposite to P' and at Q when Q is opposite to Q' respectively. Thus

$$t' = \kappa l\left(t_p - \frac{u}{c^2}x_p\right) = \kappa l\left(t_q - \frac{u}{c^2}x_q\right).$$

Whence

$$t_q - t_p = \frac{u}{c^2}(x_q - x_p),$$

$$\therefore\ x_q' - x_p' = \kappa l\,(x_q - x_p)\left(1 - \frac{u^2}{c^2}\right)$$

$$= \frac{l}{\kappa}(x_q - x_p).$$

Suppose that R is a point on S_1 such that RP is at right angles to PQ and $y_r - y_p = x_q - x_p$. Then it is easy to show that the distance RP as reckoned by people on S_2 will be $l\,(y_r-y_p)$. Hence

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[1] 'Common Sense of Relativity,' *Phil. Mag.* April, 1911.

[2] This axiom may be compared with our remarks on the Lorentz-Fitzgerald Theory, p. 374 *et seq.*

$$\frac{y_r'-y_p'}{x_q'-x_p'}=\frac{l\,(y_r-y_p)}{\frac{l}{\kappa}(x_q-x_p)}=\kappa\,\frac{y_r-y_p}{x_q-x_p}=\kappa=\frac{1}{\sqrt{1-\frac{u^2}{c^2}}}.$$

This is in accord with the Lorentz-Fitzgerald formula.

Again, the people on S_2 will not reckon two events which happen at different places on S_1 as contemporary when the people on S_1 count them as contemporary. Suppose an event happens at P on S_1 when the clock there marks t and another at Q on S_1 when the clock there marks the same. The times at which they will be judged to have happened by people on S_2 will be those marked by their clocks which are opposite P and Q respectively when the clocks at P and Q mark t. Let these times be t_p' and t_q'.

Then $t_p'=\kappa l\left(t-\frac{v}{c^2}\,x_p\right)$ and $t_q'=\kappa l\left(t-\frac{v}{c^2}\,x_q\right)$,

$$\therefore\ t_q'-t_p'=-\kappa l\,\frac{v}{c^2}\,(x_q-x_p)$$

$$=-\frac{v}{c^2}\,(x_q'-x_p').$$

So the event at the place further along the axis of x is judged by the people on S_2 to happen before that at the point nearer the origin, though the people on S_1 judge them to be contemporary. Of course the complete reciprocity of the transformation equations involves that all these remarks apply, *mutatis mutandis*, to the judgment of people on S_2 about times and distances on S_1.

But, now that we have seen some of the implications of setting clocks and measuring lengths according to the reasonable conventions set out above, the question arises: What would the people on S_1 and S_2 really believe about the physical world? We must assume that they can communicate with each other (*e.g.* by wireless telegraphy); indeed we have already assumed this when we said that they both took the same number c as the measure of the velocity of light relative to their own systems from sources fixed in them. But we will not assume at present that clocks or measuring rods can be carried about either system, or, *à fortiori*, from one system to the other.

It is clear that the people on S_1 will tell those on S_2 that the latter's clocks are not really synchronous, but that they get slower the further they are along the x-axis from the origin. And the people on S_2 will make exactly the same criticism on the clocks of S_1. Yet both have synchronised their clocks by the same method, and therefore the opinion of neither is to be preferred to that of the other. What are they to conclude? The difference arises out of the employment of two different methods of judging synchronism. The people on S_1 judge of the synchronism of *their own* clocks by the method of light signals, so do the people on S_2. But the people on S_1 apply a different test to S_2's clocks. Their test is that, if a clock of theirs at P marks t and the clock opposite to it on S_2 then marks t', then, if a clock of theirs at Q marks t, the clock opposite to this on S_2 ought also to mark t'. The argument is that the readings at P and P' are certainly contemporary and the readings at Q and Q' are certainly contemporary, but identity of reading at P and Q indicates identity of time if the clocks on S_1 are properly set, and therefore, if the clocks on S_2 were properly set, there ought to be identity of readings at P' and Q'. It is difficult to find any flaw in this test, and therefore we must conclude that there is something wrong with the test which is used by the separate systems, in fact that identity of clock-readings on a given system set out by the conventions that we have adopted is not a mark of identity of time. Since both parties cannot be right, and since neither has the slightest claim to be right as against the other, the only conclusion is that both may be wrong, and that these conventions, plausible as they may seem, do not give us a system of clocks such that identity of reading on two clocks of the same system means identity of time.

But the source of the perplexity would be quite clear to a believer in a fixed ether or absolute space. He would say: Your tests for synchronism in clocks and your measures of distance are not in general valid ones, they only are real tests for the particular case of systems that are at rest relative to the medium in which light travels. Now, when you have (as in your example) two systems in relative motion to each other, it is certain that one and possible that each of them is in motion relative to the ether. Hence your tests are bound to lead you to wrong beliefs in at least one of the systems, and the paradoxes that you find result as a matter of course. Unfortunately no experiments will detect or measure velocity relative to the ether, and therefore we cannot tell in what system, if any, the tests proposed do really ensure the synchronism of clocks.

It is clear how it happens that the test is not in general accurate. It will be remembered that, in regulating our clocks, we said that their zeros agreed if a light signal which left the standard clock when it marks t_1 reaches a clock at P on the x-axis when *it* marks t_2, is reflected, and returns to the standard clock when it marks t_3 such that $t_2 = \frac{1}{2}(t_1 + t_3)$. We can see that this test is only valid if the velocity of light relative to our system be the same in both directions. We have assumed in fact in using the test that the velocity of light relative to each system is not merely uniform in any given direction, but is the same in all directions and in both senses. Since we have set out our clocks and coordinates on this assumption it is not surprising that *our measures* of the velocity of light accord with it; but, unless the assumption be true, agreement of numerical values of measures is no proof of identity of what is measured. On the other hand *until* we have made some assumption that will enable us to set out coordinates and regulate clocks, we can get no numerical measures at all. The result of the discussion seems to me to be this: (1) Left to themselves the people on S_1 and S_2 would have no reason to doubt the correctness of the assumptions on which they regulated their clocks and set out their coordinates. (2) Even when they can communicate and discover disagreement neither has any claim to be right as against the other, since both have made the same assumptions. Hence one must be wrong and both may be, and neither can tell which is. (3) The assumption which must be wrong in at least one case is that the velocity of light relative to the system in nature (as distinct from the numerical value found for it) is the same in all directions and both senses. (4) Since you cannot regulate your clocks and set out your coordinates *until* you have made some assumption on this point, and cannot get numerical values for the relative velocities of light in various directions until you have regulated your clocks and set out your coordinates, no numerical value gained by experiment can ever without circularity be claimed as a proof of your assumption. (5) But disagreement between people on different systems who have made similar assumptions on points as to which there must be agreement if the assumptions be true disproves their truth in one case at least. (6) What remains to be said is that these disagreements are of no direct importance to philosophy or to science. They are of no direct importance to philosophy, because the reality of time and space and the existence of equal lengths and contemporary events are in no way touched by the fact that it is impossible to give an absolutely satisfactory test for equality of lengths or times. Indirectly they are of great interest because they force us to see that assumptions are made in all measurement which are often overlooked or taken as obviously true. And they are of no direct importance to science, because we are not there concerned to discover laws connecting absolute lengths, durations, and other magnitudes, but only laws connecting the values which we can read off our instruments. No doubt we call these values measures of the lengths, times, forces, etc. in nature, and so speak of the laws connecting the measured values as laws of what they are supposed to measure. But what we have to remember is that, whilst these laws do, no doubt, imply the existence of laws in nature, they are expressions of the latter subject to the assumptions on which our measured values are supposed to be measures of the natural magnitudes in question. If those assumptions be false, but internally consistent and consistently employed, we shall arrive at laws which are true and valuable, but are not direct transcriptions into numbers of the functional relations of those natural magnitudes in terms of which they are verbally stated. To put it more clearly, let us suppose that I discover a law connecting my measures of three magnitudes P, Q, and R. I shall no doubt talk of this as a law connecting the magnitudes P, Q, and R themselves. Now, all my measurements are made on certain assumptions about what is a test for equality and inequality among such magnitudes. If these assumptions be true, i.e. if my assumptions lead me to *believe* that two quantities of the kind P are equal when they *actually* have the same magnitude, and to *suppose* that one is greater than the other when it *actually* has a greater magnitude, and so on, then my law connecting the measures of P, Q, and R can be called a direct transcription into numbers of the law of nature connecting the magnitudes themselves. If, on the other hand, my assumptions be false, but consistent and consistently used, i.e. if they lead me to take quantities with different magnitudes as equal, and so on, yet so that my mistakes obey a fixed law, then the law connecting my measures will not be direct transcription into numbers of that which in nature connects the magnitudes of the kind involved, but it will be an indirect transcription of it which will be just as useful, and—since it is connected with the direct transcription in a fixed, though to us unknown, way—in a perfectly definite sense, true. The Principle of Relativity, as a physical theory based on empirical evidence, now simply comes to this. If the people on two systems S_1 and S_2 in uniform relative motion to each other have both laid out their coordinate systems and regulated their clocks on the assumptions described above, what each takes to be the measure of any given physical magnitude will be the same function of what he takes to be the measures of other physical magnitudes. We know that one man's laws, at any rate, cannot be a direct transcription of the laws of these magnitudes in nature, and that perhaps the laws of neither are direct transcriptions; but this need worry no one, since no one can ever prove that his laws are direct transcriptions of those of nature, and all purposes of science are served if they be only indirect ones.

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